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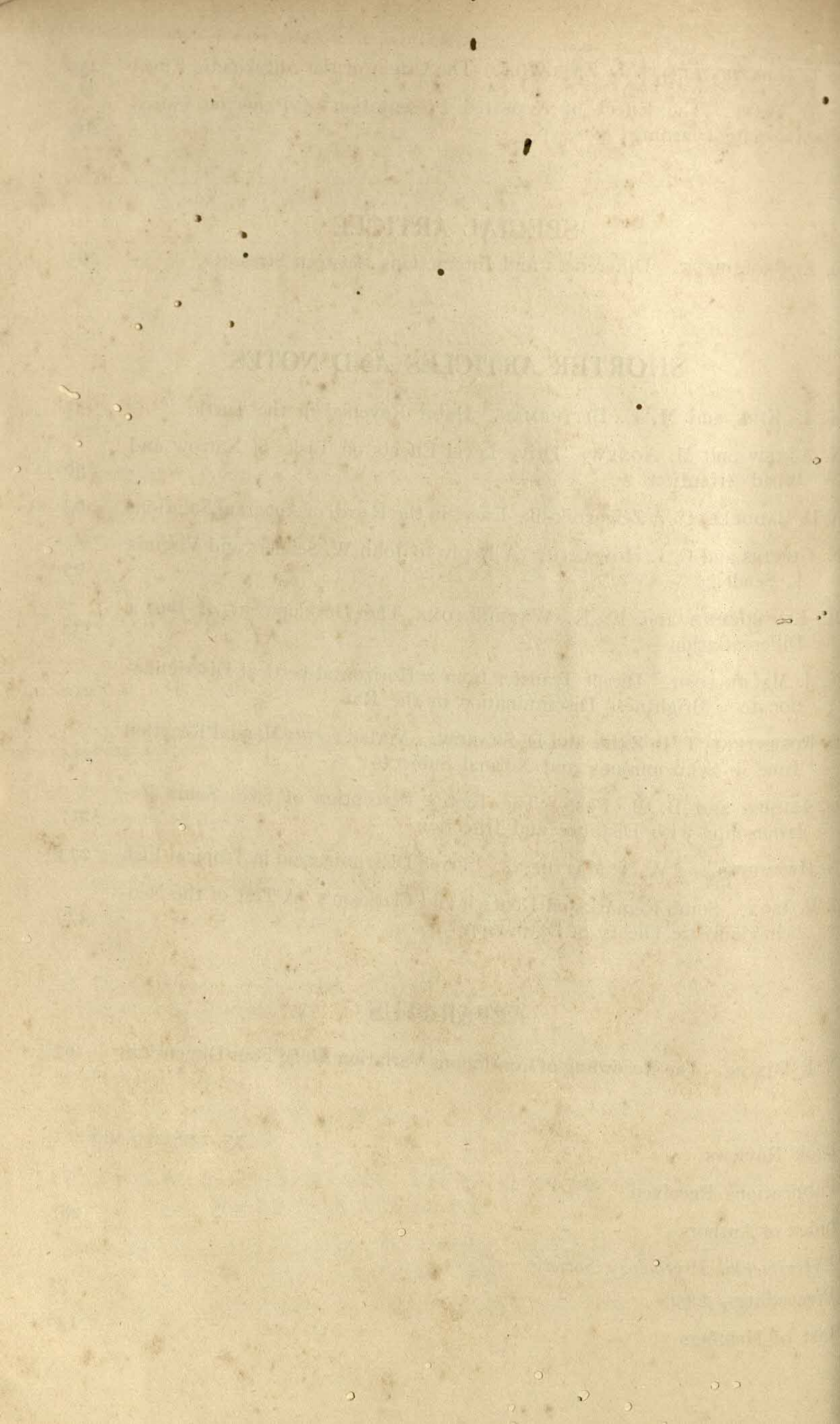
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THE QUARTERLY JOURNAL OF EXPERIMENTAL PSYCHOLOGY

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Part 1

THE REFRACTORY PERIOD AND SERIAL CHOICE-REACTIONS

BY

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An experiment is described in which subjects had to make consecutive 2-choice reactions to light and sound stimuli, the interval between signals being fixed for any one series of presentations. When under these conditions, reaction times to the second signal are compared to those obtained when the first signal is used only as a warning, delays are found for short intervals. These results and the wide individual differences obtained are discussed in relation to theories that have been put forward to account for the delays obtained in serial reaction tasks.

INTRODUCTION

The delay in the response to the second of a pair of closely spaced consecutive signals is a well established phenomenon. (Vince, 1948, 1949; Welford, 1952; Elithorn and Lawrence, 1956; Davis, 1956, 1957). It has been demonstrated, using graded responses, such as step-tracking, where feed-back from the response is necessary for correct performance, and ungraded responses, such as key pressing where feed-back, though possibly utilised, is not essential to practised subjects. Delay has been shown to occur both when the two signals are in the same sensory modality, and when they are different (Davis, 1956, 1957). The cause of delay is thus presumably of central origin.

Three types of theory have been put forward in explanation and summarised by Broadbent (1958). A theory due to Welford and elaborated by Davis, is based on the central processes concerned with two separate stimuli not being able to co-exist. When the interval between the signals is less than a given amount, queuing occurs. Davis's suggestion of a "rest period" following the processing of the first signal is a variant of this type in that delay remains a function of the first reaction time.

Another type of theory postulates a perceptual sampling process, the delay in dealing with a particular signal depending on whereabouts in a sampling period it happens to arrive. To explain a systematic difference between simple and serial reaction times, such a theory must further propose that the second of a pair of closely spaced signals arrives at a point in a sampling period which is not random. A version put forward by Broadbent involves closure of a sample immediately after the arrival of the first signal so that the delay in responding to the second signal is given by the difference between the sampling interval and the interval between signals. On such a theory delay in the handling of the second signal becomes independent of the reaction time to the first (except in so far as both reaction times might be jointly affected by factors not related to the present hypothesis).

A third type of theory regards delay as the consequence of subjective uncertainty about the time of arrival of the second signal. Thus delay is independent of the first reaction time, and indeed, of the occurrence of the first response.

Different forms of this kind of theory have been proposed by Elithorn and Lawrence (1955) and by Drazin (1960), who found that both simple and serial reaction times were primarily dependent on the distribution of the interval separating the stimulus from the immediately prior signal.

A situation in which the reaction time to the first signal is substantially longer than in Davis's experiments should distinguish between theory types 1 and 2. Long reaction times mean long central, *i.e.* non-peripheral times. Neither Welford's nor Davis's formulation suggest a sub-division of the central period, and hence longer first reaction times should mean longer delays. By contrast, on a perceptual sampling theory, the delay in dealing with the second signal would be independent of the reaction time to the first. While such independence is consistent with yet other types of theory, in the present context evidence for a fixed waiting period would favour perceptual sampling.

Theories ascribing delay exclusively to subjective uncertainty about the arrival times of the second signal must predict no delay when the interval between signals is fixed. The result of substituting uncertainty about what the second signal will be, for uncertainty about when it will arrive, should therefore be crucial to theories of type 3 in comparison with the others.

The following experiment was set up to distinguish between the three types of theory.

EXPERIMENT

Apparatus

The subject sat at a specially constructed desk with the second and fourth fingers of each hand resting on relay arms which acted as keys. The force necessary to depress these was approximately 110 gm. In a groove in front of these keys two small lamps were mounted in such a way that they could be moved directly opposite the two left-hand or the two right-hand keys. The subject wore padded earphones through which the auditory signals were provided. These were pure tones of 1,000 and 3,000 cycles respectively, and of 20 millisecon. duration.

The visual signal consisted of the flashing of either light. The lamps used were ordinary 3.5 volt filament bulbs supplied with a 12 volt pulse of 20 millisecon. duration. Since pulses of 5 millisecon. duration produced easily visible effects, it was argued that any discrepancy between the recorded time of signal arrival and the moment it became observable, were of this order, and thus within the limits of accuracy of the measuring instrument used.

Using a constant speed drum operating microswitches, the following sequence of events was produced. (1) Prepared paper tape was moved on one position in a tape reader to select a particular pair of light and sound signals for the next presentation. This was accompanied by a click easily audible through the earphones. (2) After an interval of 0.5 sec. a relay was operated, presenting the subject with his first signal. At the same time an R-C circuit was closed producing the second signal after a predetermined interval, fixed for any run of 30 consecutive presentations, and varied between runs from 50 to 800 millisecon. Signals were presented at the rate of one pair every 5.5 sec.

Any one subject always used the same hand for responding to signals in any one sensory modality. The task was to press the key immediately behind the flashing bulb, and to press the left hand key of the other pair for the lower tone, the right hand key for the higher.

Signals, responses and times were recorded (correct to the nearest 10 millisecon.) by means of a S.E.T.A.R. (Welford, 1952).

Experimental procedure

Each subject had sixteen experimental sessions each of which lasted about half an hour. Only the last eight sessions were used, the rest providing practice. Each session consisted of eight runs, these being of two kinds. (i) Double-runs in which the subject was required to respond to both signals. (ii) Single-runs in which only the second signal was responded to, the first being the same on every occasion, and different from either of those normally given—an 1,800 cycle tone, or a light moved away from

the corresponding response key. During an S-run subjects removed the hand from the keys corresponding to the first signal. The object of including "Double" and "Single" runs in the same experimental session was to remove the effect of day to day changes in performance from the difference between them. It is however possible that by disturbing the subjects' "set" the method may have introduced more variability than it avoided.

All runs consisted of 30 presentations, of which the first five were not responded to and the next five ignored in the analysis. Eight different inter-signal intervals were used: 50, 100, 200, 300, 400, 500, 600 and 800 millisecc. Writing S300 to denote an S-run with a 300 millisecc. interval between first and second signal, a typical session would take the form S50, S100, D50, D100 Rest Pause, D200, D300, S200, S300; or S400, S500, D400, D500 Rest Pause, D600, D800, S600, S800, both increasing and decreasing sequences being used. Each subject was presented with one increasing and one decreasing sequence in the 50-300 and 400-800 interval range, for both the light-sound and the sound-light orders, making 8 sequences, duplicated by a similar eight during the practice period. The order of presentation of signals were randomised separately for each session, subject to the condition that no stimulus pair could occur more than three times in succession. Each subject completed his sixteen sessions in two weeks at the rate of two sessions a day, the last eight being completed on four consecutive days. In all cases subjects were instructed to respond to each signal as quickly as possible.

The automatic method of presentation used, as well as the method of recording, made it impossible to provide subjects with immediate knowledge of results. This may be the reason for the comparatively large variance in the results, and as such an important shortcoming of the experimental method (see discussion).

RESULTS

Three right-handed subjects were used, of whom A and B used the right hand for responses to light and C the left hand. Their results are given in Table I.

TABLE I
MEAN VALUES AND STANDARD ERRORS OF RT_1 , RT_2 (DOUBLE) AND
 RT_2 (SINGLE) IN MILLISEC.; AND VALUES OF $\rho(RT_1, RT_2 \text{ DOUBLE})$

Subject A — Light-Sound

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT_1	Mean	..	490	530	620	700	260	270	270	240		260		320	720	630	640	700
	S.E.	..	25	22	25	40	8	9	9	9		8		9	14	17	25	36
	N	..										(19)						
RT_2 Double	Mean	..	420	420	410	390	320	340	310	290		290		340	410	420	520	640
	S.E.	..	25	22	34	39	13	18	15	13		13		13	15	18	24	34
	N	..						(19)							(19)		(19)	
RT_2 Single	Mean	..	250	270	260	240	290	330	270	300		280		300	280	290	310	250
	S.E.	..	14	21	14	9	14	14	9	18		8		10	15	19	16	8
	N	..	(19)												(19)			(19)
$\rho(RT_1, RT_2(D))$			0.97	0.96	0.78	0.99	0.51	0.14	0.08	0.26		-0.18		0.16	0.99	0.93	0.99	0.99

Subject A — Sound-Light

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT_1	Mean	..	550	610	600	340	260	270	250	250	250	260	270	300	260	310	540	530
	S.E.	..	20	25	23	20	9	16	10	8	9	11	11	25	7	14	25	29
	N	..							(19)									
RT_2 Double	Mean	..	520	520	420	310	230	250	240	220	260	250	260	310	270	310	460	490
	S.E.	..	20	27	24	15	7	11	6	6	9	8	10	22	61	15	25	30
	N	..	(19)		(19)									(19)				
RT_2 Single	Mean	..	230	260	250	240	250	250	240	250	260	250	250	260	230	240	250	240
	S.E.	..	9	13	8	9	11	7	6	8	9	9	9	10	12	7	10	9
	N	..												(19)				
$\rho(RT_1, RT_2(D))$			0.99	0.97	1.00	0.96	0.74	0.53	0.25	0.08	-0.25	0.27	0.20	0.77	0.74	0.76	0.97	1.00

Subject B — Light-Sound

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT ₁	Mean	..	320	300	300	280	260	260	270	260	300	300	260	280	300	270	290	320
	S.E.	..	9	9	9	9	7	12	9	7	13	12	11	13	10	8	15	18
	N	..	9	9	9	9	(18)	(19)	9	7	13	12	11	13	10	8	15	18
RT ₂ Double	Mean	..	540	490	430	420	340	330	370	320	340	340	370	360	360	400	460	550
	S.E.	..	20	24	16	21	24	17	23	13	17	16	23	21	19	20	27	18
	N	..	(19)	(18)	(18)	21	24	17	(19)	13	17	16	(19)	21	19	(18)	(18)	(19)
RT ₂ Single	Mean	..	290	330	310	330	270	310	350	320	330		340	350	280	280	350	300
	S.E.	..	16	23	6	17	11	13	31	22	23		16	15	19	18	12	14
	N ₀	..	16	23	6	17	11	13	31	22	23		(19)	15	19	18	(19)	14
$\rho(RT_1, RT_2(D))$			0.62	0.27	0.20	0.45	0.20	0.20	0.28	-0.08	-0.06	0.12	0.53	0.26	0.37	0.33	0.56	0.64

Subject B — Sound-Light

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT ₁	Mean	..	300	370	380	340	310	280	330	340	300	300	330	340	340	350	410	350
	S.E.	..	21	26	26	27	18	16	14	15	11	13	15	27	14	20	27	21
	N	..	21	26	26	27	(19)	16	14	15	11	(19)	15	27	(19)	20	27	21
RT ₂ Double	Mean	..	360	420	370	320	270	250	280	240	280	260	280	260	270	340	450	360
	S.E.	..	23	25	19	23	11	8	9	9	18	9	12	19	9	22	32	20
	N	..	(18)	25	19	(18)	11	8	(18)	9	18	9	(19)	(18)	(19)	22	32	(19)
RT ₂ Single	Mean	..	260	260	260	260	260	230	270	270	290	270	320	290	270	260	280	270
	S.E.	..	14	14	9	10	11	8	10	12	13	12	15	16	11	9	11	10
	N	..	(19)	14	9	10	11	(19)	10	(19)	(19)	12	15	(19)	11	(18)	11	10
$\rho(RT_1, RT_2(D))$			0.91	0.96	0.89	0.89	0.31	-0.33	0.00	-0.12	0.00	0.17	-0.31	0.82	0.53	0.91	0.93	0.89

Subject C — Light-Sound

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT ₁	Mean	..	280	290	290	260	240	240	330	260	230	240	230	240	250	240	280	290
	S.E.	..	8	10	8	16	9	12	12	10	9	9	9	10	9	8	10	11
	N	..	8	10	8	16	(19)	12	(18)	10	(19)	(19)	9	(19)	9	8	10	11
RT ₂ Double	Mean	..	360	280	310	250	230	220	250	240	220	220	230	240	240	300	330	310
	S.E.	..	13	13	10	20	10	11	11	9	11	10	7	9	8	12	12	12
	N	..	(19)	(19)	(19)	(18)	(19)	(18)	(18)	9	(18)	(19)	7	(17)	(19)	(18)	(19)	(17)
RT ₂ Single	Mean	..	210	210	240	220	210	230	230	240	220	230	210	220	190	220	200	200
	S.E.	..	12	9	13	12	10	10	12	11	8	8	8	8	9	14	11	10
	N	..	12	9	13	12	10	10	12	11	8	8	8	8	9	14	11	10
$\rho(RT_1, RT_2(D))$			0.53	0.45	0.39	0.58	-0.03	-0.26	0.04	0.02	0.42	0.28	0.40	0.12	0.23	0.66	0.40	0.81

Subject C — Sound-Light

			Increasing								Decreasing							
			50	100	200	300	400	500	600	800	800	600	500	400	300	200	100	50
RT ₁	Mean	..	220	240	230	230	230	230	240	210	230	220	230	270	240	240	220	220
	S.E.	..	11	8	7	5	8	9	11	11	8	6	8	12	10	10	6	8
	N	..	11	(19)	(19)	5	(19)	9	(19)	(19)	(19)	6	8	(19)	10	(19)	6	8
RT ₂ Double	Mean	..	310	280	250	220	220	210	240	220	250	210	240	230	210	270	260	260
	S.E.	..	14	7	8	8	13	10	9	11	10	9	8	10	9	8	8	9
	N	..	(18)	7	8	8	(18)	10	(19)	11	10	9	8	10	(17)	(19)	(19)	9
RT ₂ Single	Mean	..	230	220	230	230	210	220	220	210	210	230	230	230	220	220	210	230
	S.E.	..	7	7	12	10	9	9	12	8	6	10	8	10	9	12	11	10
	N	..	7	(18)	(19)	(19)	(19)	9	(18)	8	6	10	(18)	(19)	(18)	(18)	(17)	10
$\rho(RT_1, RT_2(D))$			0.81	0.72	-0.03	0.20	0.15	0.24	0.16	0.11	-0.24	0.11	0.26	0.62	0.00	0.14	0.69	0.34

Means and S.E.'s are based on samples of 20 observations, except where indicated. Values of ρ are based on those pairs of observations of RT₁ and RT₂ (Double) in which no error occurred.

Means and standard errors are based on 20 observations, except where errors occurred. Response times for wrong responses are not included in the data, and in such cases, results are calculated on the reduced sample. (A small part of the records for A and B was lost as the result of an apparatus failure).

There are considerable variations both within the performances of individual subjects, and between subjects. A comparison of individual records suggests that these differences are not only quantitative but involve different methods of dealing with the incoming signals. The implication of these differences is discussed below. In view of their existence, the only really general observations that can be made are:—

1. A comparison between the reaction times to the second signal

(a) when it is the only signal responded to (RT_2 Single)

(b) when both responses are made (RT_2 Double)

shows a clear delay (RT_2 Double— RT_2 Single) for short intervals, decreasing to zero or near zero as the interval lengthens.

2. There is no indication that RT_2 (Single) is longer for short intervals than long ones. That is to say, when the first signal is kept constant, requires no response, and is used in effect as a warning signal, the response time to the second signal is not affected by the interval between signals.

Two further results are given below, neither of which however is without exceptions:—

1. Eight of the records show a lengthening of the first reaction time at short intervals. The reason for this appears however to be different in the case of subjects B and C on the one hand and A on the other.

2. There is evidence that the correlation between first and second reaction times is higher for short intervals than for long ones.

The relationship between delay and interval shows marked individual differences and can only be discussed on an individual basis.

DISCUSSION

The somewhat bewildering variety of the results make them unlikely to provide evidence for any detailed theory. They do, however, provide some negative evidence. Considering first of all theories depending on temporal uncertainty—such theories would predict that under the present conditions there would be no delays at any value of the intersignal interval, since this interval is fixed and known for any one run. Against the objection that ten consecutive presentations may not be enough to remove uncertainty about the time of arrival of the second signal, two points can be made: (i) The residual uncertainty should not be affected by whether or not the first response is made; (ii) The residual uncertainty should if anything be greater for long intervals than for short ones. The existence of a clear difference between RT_2 (Double) and RT_2 (Single) for short intervals and the absence of such a difference for long intervals seems to rule out any explanation based on temporal uncertainty only. It does not, however, rule out the possibility that where there is temporal uncertainty, this may make an important contribution to the delays obtained.

The relationship between the first and second reaction times is relevant to a distinction between theories of type 1 and type 2. If the central processes relating to the two signals cannot coexist, then the amount of delay (queuing time) must depend on the duration of the central process for the first signal.

In particular, Davis's formula for the delay is:—

$$\text{Delay} = CT_1 + CRT_1 - I + (ST_1 - ST_2)$$

as long as the R.H.S. is > 0

where CT_1 = Central time for the 1st signal.

CRT_1 = Central Refractory time, *i.e.* a refractory period extending beyond the duration of CT_1 .

ST_1 ST_2 = Sensory conduction times for the 1st and 2nd signals respectively.

I = Interval.

The assumption of a fixed central waiting period such as would be obtained with perceptual sampling gives:—

$$\text{Delay} = \text{Constant} - I + (ST_1 - ST_2)$$

if the waiting period is timed from the moment the first central period begins.

If we make the assumption the differences in reaction time are predominantly of central origin, then the first equation makes the delay dependent on RT_1 , and the second independent.

The second reaction time can, on the queuing model, be regarded as made up of a "normal" portion, whose duration is assumed independent of the first reaction time, plus the delay. Davis's formula would thus predict that:—

- (a) For small values of I , an increase in the mean value of RT_1 should produce an approximately equal increase in RT_2 .
- (b) There should be a positive correlation between RT_1 and RT_2 decreasing to zero as the interval increases.

By contrast, with a fixed waiting period variations in RT_1 should not affect RT_2 , though it is of course possible that there are other factors which make for a joint variation of RT_1 and RT_2 . An examination of Table I shows that for the shortest intervals there is in fact a significant correlation between RT_1 and RT_2 for subjects B and C, and for all types of sequences. (The results for subject A though giving high correlations, are irrelevant here since he deals with the task in a fundamentally different way. His record is discussed below.) For the longer intervals correlations are with very few exceptions not significant. These results—though unreliable because of the doubtful normality of the distributions involved—favour the Welford type model.

Davis (1957) using a sound-light sequence, and simple reactions with temporal uncertainty, obtained delays of the order of $(170 - I)$ millise., his RT_1 's being around 150 millise. (He accounts for delays of this magnitude by postulating a central refractory time— CRT_1 —extending beyond the duration of the "central process" relating to the first signal.) In the present experiment, with RT_1 's considerably in excess of 150 millise. we should expect a corresponding increase in the delays of the second response.

Using the same estimates of peripheral conduction times as Davis,

$$ST \text{ (Sound)} = 10 \text{ millise.}$$

$$ST \text{ (Light)} = 40 \text{ millise.}$$

$$PT \text{ (Motor time)} = 20 \text{ millise.}$$

we can estimate CT_1 by subtracting the total peripheral time from RT_1 . Substituting these values in

$$\text{Delay} = CT_1 + CRT_1 - I - (ST_1 - ST_2)$$

and taking $CRT_1 = 100$ millise. the value used by Davis, we can predict delays for different values of I .

• Alternatively, if we assume that after the start of the central process relating to the first signal there is a fixed period of the same order as that obtained by Davis, *i.e.* 200 millise., during which no second signal can be dealt with, then

$$\text{Delay} = 200 - I + (ST_1 - ST_2)$$

giving a second set of predictions.

Table II compares observed values of the delay with the two sets of predictions (subjects B and C only).

It is obvious from inspection of this table that despite the comparatively large standard errors of the present results, both sets of predicted values are very different from the observations. This difference does not lie merely in the average values of the delay, but also in the pattern of changes in delay with changes in the interval. There are several cases where the delay at the 100 millise. interval is greater than at the 50 millise. interval. The comparatively fast second reaction for very short

TABLE II
OBSERVED AND PREDICTED VALUES OF THE DELAY
RT₂ (DOUBLE) - RT₂ (SINGLE)

	Subject B											
	Light-Sound						Sound-Light					
	Increasing			Decreasing			Increasing			Decreasing		
	50	100	200	200	100	50	50	100	200	200	100	50
Observed ..	250	160	120	120	110	250	100	160	110	80	170	90
Predicted (1) ..	340	270	170	140	260	340	290	310	220	190	350	340
Predicted (2) ..	180	130	30	30	130	180	120	70	0	0	70	120
(1) - 0.. ..	90	110	50	20	150	90	190	150	110	110	180	250
(2) - 0.. ..	-70	-30	-90	-90	20	-70	20	-90	-110	-80	-100	30

	Subject C											
	Light-Sound						Sound-Light					
	Increasing			Decreasing			Increasing			Decreasing		
	50	100	200	200	100	50	50	100	200	200	100	50
Observed ..	150	90	70	80	130	110	80	60	20	50	50	30
Predicted (1) ..	300	260	160	110	250	310	210	180	70	80	160	210
Predicted (2) ..	180	130	30	30	130	180	120	70	0	0	70	120
(1) - 0.. ..	150	170	90	30	120	200	130	120	50	30	110	180
(2) - 0.. ..	30	40	-40	-50	0	70	40	10	-20	-50	20	90

Predictions are based on two models: (1) $\text{Delay} = CT_1 + CRT_1 - I + (ST_1 - ST_2)$;
(2) $\text{Delay} = 200 \text{ millise.} - I + (ST_1 - ST_2)$.

intervals has been noted by several workers and the suggestion has been made that at such short intervals the signals may be "grouped" and responded to as one. Broadbent's objection, that such grouping should not lead to a reduction in RT_2 , since the same amount of information is being transmitted, would be inapplicable only if the queuing time of the second signal extended beyond the duration of CT_1 —this extra "truly refractory" period would be avoided by dealing with one signal of greater information content. However, in the records under consideration, the second signal is generally dealt with before the end of CT_1 . A further objection to the suggestion of grouping in the present case is that responses are in fact made separately. (This is not the case for subject A). In the cases where this initial reversal does not take place the rate at which the delay decreases is less than would be predicted by either queuing formula.

The record of subject A raises problems of a different kind. For intervals of up to 300 millise. for light-sound sequences, and up to 100 and 200 millise. respectively for the sound-light sequences, this subject responded to both the first and second signal simultaneously. First and second reaction times were thus connected by the equation:—

$$RT_1 = RT_2 + I$$

No specific instructions were given to avoid this method of dealing with the signals except insofar as it was contained in the instruction to respond to each signal as quickly as possible.

If one assumes that the peripheral conduction times involved in a reaction are not affected by this method of response, then clearly in this case the "central processes" corresponding to the two signals must coexist except for the portion falling within the interval. If Welford's hypothesis is to be retained, the "central process" referred to cannot be co-extensive with what is left over when peripheral conduction is removed from the total reaction process. The subdivision of the central process is already implicit in the notion of "queuing" by the second signal, this queuing being both central and yet co-existing with the central part of the first response. When, as in the present case, both responses occur simultaneously, that is, the first response is held up, some sort of waiting, possibly of a different kind, occurs, and this can happen at the same time as dealing with another signal. Broadbent's (1958) information-flow model contains among other units a short term store and a limited capacity channel, in which the latter would account for the need to queue and the former provide a place to do it in.

However, Broadbent's short term store is a store for signals, whereas we have spoken of the first *response* being held up. When consecutive signals are responded to simultaneously, does the subject store the first *signal* until the arrival of the second, and then translate this joint signal into a joint response, or does he decide on a response to the first signal, and then *store the response* in some way until the second signal has been dealt with? We might consider a modification of Broadbent's model in which the limited capacity channel is regarded as effecting the transition between some internal representation of stimulus-category, and a representation of the appropriate response category, with a short term store at either end. If we now suppose that this channel can deal with only one S-R translation at a time, it will correspond to the "central process" in Welford's sense.

If such a system is used to produce simultaneous responses to consecutive signals, then for intervals less than the first central time, different delay patterns are obtained according as storage takes place in the first and second store.

Consider first the case in which signal 1 is dealt with on arrival and the appropriate response is stored. (This situation is represented in Figure 1a. Peripheral

FIGURE 1(a)

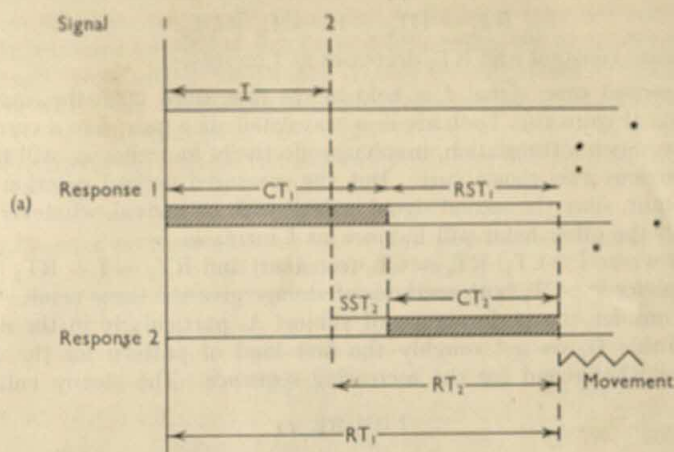
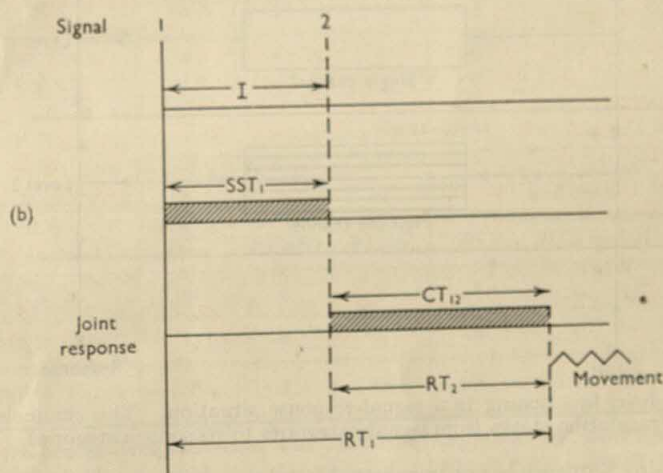


FIGURE 1(b)



I Interval between signals.

CT_1, CT_2, CT_{12} Time for which central channel is occupied.

SST, RST Waiting time in sensory store and response store respectively.

RT_1, RT_2 Overall measured reaction times.

For explanation, see text.

conduction times are omitted for simplicity.) If a second signal arrives in the first store before the central channel has been cleared, it will have to queue, on Welford's hypothesis, *i.e.* the second response will be delayed by an amount equal to the time for which the central channel is occupied by the first signal minus the interval between signals.

$$\begin{aligned} RT_2 &= SST_2 + CT_2 \\ &= CT_1 - I + CT_2 \end{aligned}$$

But since the responses occur at the same time,

$$RT_1 = RT_2 + I = CT_1 + CT_2$$

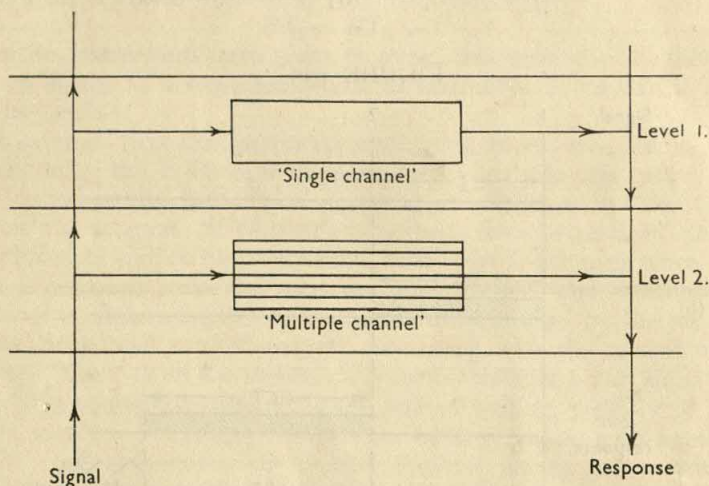
i.e. RT_1 remains constant and RT_2 decreases as I increases.

In the second case, signal 1 is held in the first store until the arrival of the second signal. (Figure 1b). Both are now translated, as a pair, into a corresponding response pair. Such a translation, involving effectively four choices, will take longer than the previous two choice case. But the measured second reaction time will now be constant, since the second signal is dealt with on arrival, whatever the value of I . RT_1 on the other hand will increase as I increases.

In cases where $I > CT_1$, $RT_2 = CT_2$ (constant) and $RT_1 = I + RT_2$ (increasing with I). Thus for $I > CT_1$ both methods of storage give the same result.

If we consider the performance of subject A, particularly in the light-sound sequences (Table I), we get roughly the first kind of pattern for the decreasing sequence, and the second for the increasing sequence. The steady value of RT_2

FIGURE 2



A model of 2-level functioning in a signal-response situation. The channels represent translation stages from signal-categories to response-categories.

in the increasing sequence is not unreasonable for a 4-choice reaction time (0.4 sec.). Furthermore, it is plausible that a sequence which starts with a very short interval between signals would give rise to the method involving the *grouping of signals*, whereas the sequence starting with the longer intervals would start off with the method which dealt with the first signal on arrival.

In the sound-light sequence, the change-over from paired to independent responses occurs at a shorter interval (300 and 200 millisec. respectively). But the data for paired responses is not inconsistent with the assumption that a response-storing method is being used. If we allow the possibility of these two types of storage, and exclude time-in-store from that part of the central process which must be exclusive to one signal, the results of subject A fit a queuing model reasonably well.

Any attempt to use results as diverse as those obtained in the present experiment as the basis of an explanatory model must be extremely tentative. It has been demonstrated that none of the models already put forward satisfactorily account

for the results, either because they make fundamentally different predictions, such as the models based on temporal uncertainty, or because they are not sufficiently complex to suggest how anomalies such as those observed might be produced—a shortcoming strongly emphasized by Drazin (1960). A suggestion is made below of a direction in which the complexity of existing queuing models might be increased.

It may be that the transition from signal to response can in fact take place at two levels, one involving a single channel and producing delays, the other involving

TABLE III
ERRORS IN RESPONSES TO THE FIRST AND SECOND SIGNAL
(DOUBLE AND SINGLE)

	<i>Light-Sound</i>					
	<i>I = 300 millisecc. and below</i>			<i>I = 400 millisecc. and above</i>		
	<i>RT₁</i>	<i>RT₂(D)</i>	<i>RT₂(S)</i>	<i>RT₁</i>	<i>RT₂(D)</i>	<i>RT₂(S)</i>
Subject A	0	2	3	1/120	1/120	0/120
Subject B	0	10	1	3	2	2/140
Subject C	0	12	7	6	12	4

	<i>Sound-Light</i>					
	<i>I = 300 millisecc. and below</i>			<i>I = 400 millisecc. and above</i>		
	<i>RT₁</i>	<i>RT₂(D)</i>	<i>RT₂(S)</i>	<i>RT₁</i>	<i>RT₂(D)</i>	<i>RT₂(S)</i>
Subject A	0	2	1	1	1	3
Subject B	1	6	4	2	5	4
Subject C	3	7	13	5	3	7

Errors are pooled over increasing and decreasing sequences and over interval values 50—300 millisecc. and 400—800 millisecc. Except where indicated, errors are out of a total of 160 responses.

several channels and no delays. The results of reaction time experiments with high stimulus response compatibility (*e.g.* Leonard, 1959) showing reaction time under these circumstances to be independent of the information content of the signal, make the suggestion of such a second level of operation plausible. High stimulus-response compatibility means a high degree of association between the two, such as would be produced by prolonged practice. With moderate amounts of practice both levels of operation might be employed. (There is a parallel phenomenon at certain stages of the development of more complex motor skills). Such a mixture of methods could account for the irregularity of changes in delay.

The model illustrated in Figure 2 represents a situation in which S-R translations are normally made via a limited capacity channel at level 1, the time taken depending on the information content of the signal, but can, after long practice be made at a second level, where the "connection" between signal and response is of a

more direct kind and acts effectively as a channel of unlimited capacity. If such a model is taken as a basis for considering the present results, subject A may be regarded as operating predominantly at level 1, with the others using level 2 much more extensively, especially subject C.

It should be noted in this connection that subject A made considerably fewer errors than either B or C (Table III).

If we regard operation at level 2 as being the result of extensive practise, then a comparatively early use of this level would produce more mistakes.

One interesting consequence of the model is the possibility of consecutive similar and different responses to a single signal. It has been noted repeatedly that when errors occur, they are corrected after an interval which is much less than a normal reaction time, such as would be required if feedback information was being used. A detailed examination of subjects' errors is now being carried out to obtain error correction times, and to check on the "correction" of correct responses. An experiment is planned to examine subjects' finger pressure on response keys, since the model suggests that the "instruction" to carry out a particular response might in fact occasionally be given twice. What is of interest here is not so much the performance of highly practised subjects, as performance at different learning stages. Further work is also needed on simultaneous response to consecutive stimuli, to test further the possibility of responses being stored as well as signals.

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VISUAL DISCRIMINATION IN THE TOAD

BY

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Using a method of successive discrimination, 10 toads (*Bufo bufo*), were trained to discriminate between horizontal and vertical rectangles each 5.0×7.5 cm. After training, one optic nerve was severed in certain animals and the stumps pushed well apart to delay regeneration. The non-operated animals retained their learning over the rest-period of 10 weeks at a level similar to training. The animals with one eye showed considerably less retention than the non-operated animals.

INTRODUCTION

Several authors have shown that frogs and toads can learn simple mazes and detours (Buytenijk, 1918; Franz, 1927; Yerkes, 1903) and Franz has suggested that toads show quicker learning than frogs. There has been little attempt, however, to train these animals to make specific visual discriminations or to observe their capacity for the retention of learned visual responses. Yerkes (1903) trained frogs to learn a maze in which two cards of identical shape but different colour (one red, one white) were important visual cues. When these cues were reversed the animals were unable to relearn the maze. Pache (1932) showed that frogs can differentiate between several different pairs of moving shapes, but that only one animal out of 14 tested (a *Hyla*) could learn to respond differentially to a motionless triangle and a circle. The importance of movement for eliciting an attack has also been shown by Franz (1927), who found that toads did not snap at a motionless mealworm even after months of training with moving mealworms.

In carrying out work on similar lines to Pache's there are several problems involved. First is the fact that towards the latter part of Pache's experiment a mere turn of the head by the animal in the direction of either of the two shapes was scored as a response. (In Pache's experiment mealworms were attached to the positive stimuli and under the experimental conditions, feeding responses became inhibited and the frogs ceased to approach the shape.) A second problem is that the figures discriminated by Pache's animals differed in area and contour so that the animals may have been discriminating solely on the basis of total surface area as opposed to outline.

In view of these findings and their limitation it was decided to make a further study of amphibian form discrimination by designing a different technique from that of Pache and using toads as subjects on the assumption that they would probably be better learners than frogs.

METHOD

Apparatus and method of recording

The animals were trained to discriminate between two figures, equal in shape and area; a horizontal and a vertical rectangle, each 7.5×5.0 cm. cut out of black perspex and sandpapered to give a matt surface. The size of the figure was chosen arbitrarily in conjunction with the design of the box but approximated with the size of figures used by Pache and the coloured shape used by Yerkes. The fact that a figure with a straight edge was chosen is now of increasing interest in view of Lettvin *et al.* (1959, 1960) findings (see Discussion).

A method of successive discrimination was used whereby both placing the animal in the direct glare of artificial light and hindering the animal's escape by an electric shock constituted punishment, and escape to water and darkness in the home tank without

shock constituted reward (Curtis, 1913; Yerkes, 1903). The figures were presented in varied order at the neck end of a V-shaped escape box and were attached to a motor by a brass arm which moved up and down continuously (see Fig. 1). The extent of this vertical movement was 3 cm. and one complete up/down movement took approximately 2 sec. The open box, painted white inside and with the corners rounded, was illuminated from above by a 100-watt shaded lamp. The wall of the box was 18.0 cm. high, the sides 30.0, 38.0 and 40.0 cm. long. The bulb of the lamp was placed centrally and approximately 12 cm. above the top edge of the wall; hence the alley way opening appeared dark by contrast.

In order to pass by the figure and escape into the tank the animals had to cross a 1.80 cm. long wire grid. This grid was electrically charged during trials in which the

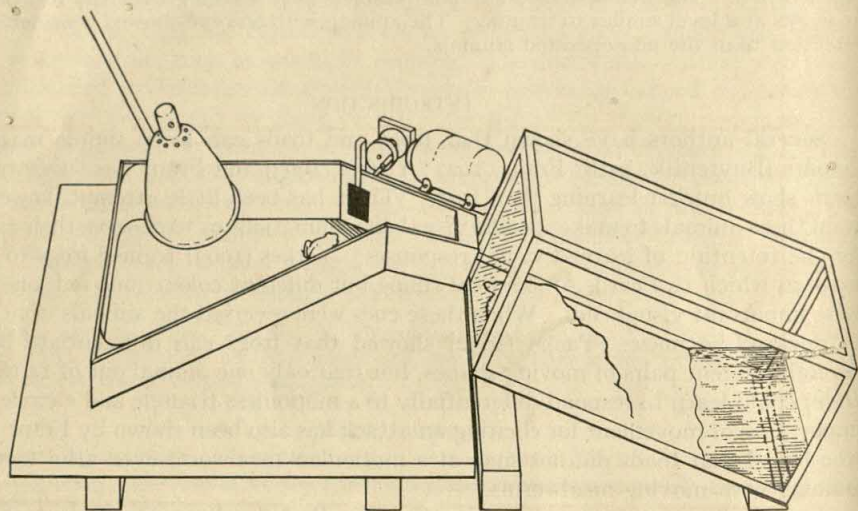


FIGURE 1

Diagram of apparatus designed to demonstrate successive discrimination in the toad. The animals were trained to pass by a moving figure in order to escape from an illuminated area into a dark tank with water and peat. When the "negative" figure was presented they received shocks from a wire grid just below the figure: when the "positive figure" was presented they were allowed to escape without shock. The difference in time between the positive and negative trials indicates the degree of discrimination present.

negative figure was presented and so delayed the animal's escape. Times were recorded for the animals (1) to touch the grid and therefore receive a shock during negative presentations. If, after receiving a shock, the animal retreated from the grid instead of escaping it was removed from the box. (2) To place both forelegs on the grid during positive trials. If an animal did not escape after 5 min. it was pushed gently through the alley way. The same criteria were not used for recording positive and negative trials because during positive trials animals sometimes just touched the edge of the grid and then turned back into the box.

Mean scores per trial for responses to the positive and negative figures separately were calculated after each day's training. The difference ($\pm$) between these two scores was then calculated and will be referred to as the mean difference score. (For further details of the training technique see Appendix A.)

PLAN OF EXPERIMENT

Preference tests

A group of six animals was run for five days without shocks to observe whether any innate preference existed for either of the figures to be discriminated by the trained

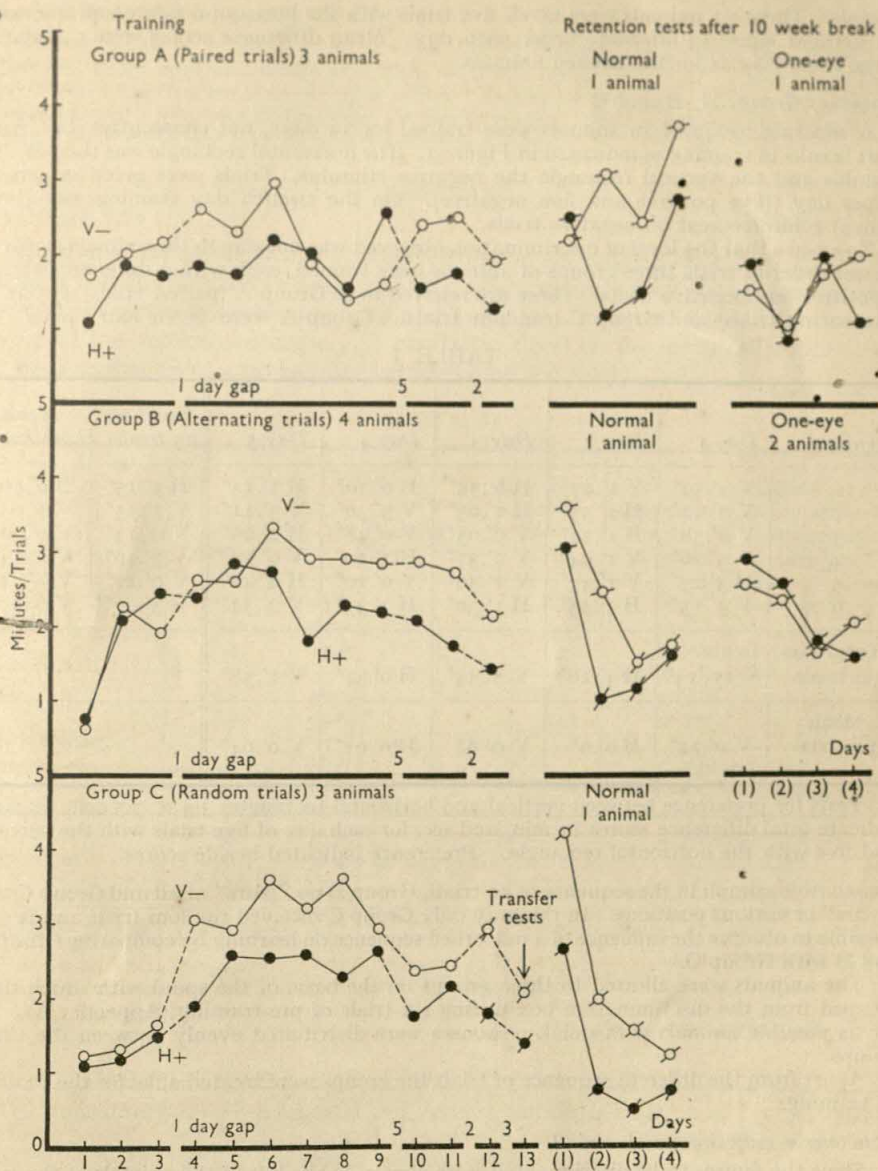


FIGURE 2

Mean times per trial for three groups of animals each trained with a different sequence of positive and negative trials.

○ = mean times per trial with the "negative" figure (vertical rectangle 5 × 7.5 cm.)

● = mean times per trial with the "positive" figure (horizontal rectangle 5 × 7.5 cm.)

● = retention tests, given without shock after a 10-week's break. These tests are shown separately for animals with one optic nerve cut at the end of training.

animals. These six animals were given five trials with the horizontal figure and five with the vertical figure in alternate order each day. Mean difference scores were calculated in the same way as for the trained animals.

Training; Groups A, B and C

A separate group of 10 animals were trained for 12 days, not consecutive but with short breaks in training as indicated in Figure 2. The horizontal rectangle was the positive stimulus and the vertical rectangle the negative stimulus. Trials were given in sets of 10 per day (five positive and five negative). On the twelfth day training was given without reinforcement on negative trials.

To ensure that the level of discrimination observed was not due to the animals learning any set order of trials three groups of animals were trained, each with a different pattern of positive and negative trials. These are referred to as Group A (paired trials), Group B (alternating trials) and Group C (random trials). Group A were given four "pairs" of

TABLE I

<i>Animal</i>	<i>Day 1</i>	<i>Day 2</i>	<i>Day 3</i>	<i>Day 4</i>	<i>Day 5</i>	<i>Total for 25 trials</i>	<i>Mean per trial</i>
1	V 4'.56"	V 1'.07"	H 6'.55"	H 0'.10"	H 1'.13"	H 2'.15"	H 0'.06"
2	V 0'.12"	H 2'.55"	H 2'.03"	V 3'.30"	V 3'.11"	V 1'.55"	V 0'.05"
3	V 2'.58"	H 1'.30"	V 0'.05"	V 0'.48"	H 2'.06"	V 9'.15"	V 0'.22"
4	V 3'.20"	V 1'.04"	V 2'.37"	H 1'.29"	V 0'.14"	V 5'.46"	V 0'.14"
5	H 3'.27"	V 1'.41"	V 2'.30"	V 0'.20"	H 0'.42"	V 0'.22"	V 0'.01"
6	V 3'.15"	H 3'.53"	H 1'.30"	H 3'.33"	V 2'.34"	V 3'.07"	V 0'.07"
Total for 30 trials	V 11'.14"	H 4'.26"	V 3'.44"	H 0'.34"	V 1'.58"		
Mean per trial	V 0'.22"	H 0'.9"	V 0'.8"	H 0'.01"	V 0'.04"		

Tests for preference between vertical and horizontal rectangles 3.5×7.5 cm. Figures indicate total difference scores in min. and sec. for each day of five trials with the vertical and five with the horizontal rectangle. Preference indicated beside scores.

consecutive stimuli in the sequence of 10 trials, Group B no "pairs" at all and Group C two "pairs" in various positions. In this way only Group C received random trials and it was possible to observe the influence of a patterned sequence on learning by comparing Group A and B with Group C.

The animals were allotted to three groups on the basis of the speed with which they escaped from the discrimination box during six trials of pre-training (Appendix A). So far as possible animals with quick responses were distributed evenly between the three groups.

Apart from the different sequence of trials the groups were treated alike for the 12 days of training.

Optic nerve cut: Groups A and B

Since the figures to be discriminated were always to the left of the animals at the time of escape (Fig. 1) it was possible that the animals would learn predominantly with the left eye. Hence immediately after training the right optic nerve was sectioned in one animal from Group A and two from Group B to observe whether this affected their capacity for retention of the learned response (see Table II). The dural sheath was completely severed and the stumps pushed well apart to prevent the nerve fibres regenerating; in one animal a short piece of nerve was completely resected.

Transfer tests: Group C

Three days after training Group C were given transfer tests for one day, without shock, to a larger rectangle than the one with which they had been trained but with approximately the same proportion of length to breadth, i.e. 6.5×9.5 cm. instead of 5.0×7.5 cm.

Retention tests after 10 weeks

After a break of 10 weeks from the last day of training six animals were tested for retention; three normal animals and three with one optic nerve cut. These six animals were given a random sequence of trials without shocks for four days. These test days are referred to with numbers in brackets, i.e. days (1)-(4). The remaining four animals, all normals, unfortunately died during the retention period.

RESULTS

Preference tests

No significant preference was observed in the six animals described above (see Table I) (i.e. the animals escaped equally quickly whether they were presented with the vertical or the horizontal rectangle.) There was, however, a slight tendency in favour of the vertical rectangle. (A two-tailed *t* test on the mean difference scores for each animal gives a probability between 0.05 and 0.10.)

TABLE II

Groups	Animals	Training Days 4-12	One optic nerve cut	Retention after 10 weeks	Difference in score between training and retention
Paired	A1	15.0	A1*	7.8*	- 7.2*
	A2	21.0		53.4	32.4
	A3	14.0		Animal died	
Alternating ..	A4	36.3	A5*	37.1	0.8
	A5	59.8		22.1*	- 37.7*
	A6	26.0		Animal died	
	A7	19.3		- 30.8*	- 50.1*
Random ..	A8	48.8		Animal died	
	A9	50.4		Animal died	
	A10	27.1		66.1	39
Combined Means	1. Normals (3)	28.1	1. Normals (3)	52.3	mean gain 24.2
	2. Animals to be operated (3)	31.4	2. Animals with one optic nerve cut (3)	- 0.3	mean drop - 31.7

Discrimination between horizontal and vertical rectangles each 5.0 × 7.5 cm. Mean difference scores in sec. for each animal throughout 8 days of training and four days of retention tests; the latter given after a 10-week break.

* = animals with one optic nerve cut at the end of training.

Training: Days 4-12

1. For each group of trained animals the mean times taken to pass the positive figure were significantly less than those taken to touch the live wires when the negative figure was presented ($p = 0.02$ for each group; one-tailed *t* test). Total mean difference scores: group A (paired trials) 16.1 sec., group B (alternating trials) 35.4 sec., group C (random trials) 41.6 sec. (see Table II). These mean difference scores are not high largely because performance was often erratic (see Fig. 3). It must be remembered, however, that the animals make the same response to both the

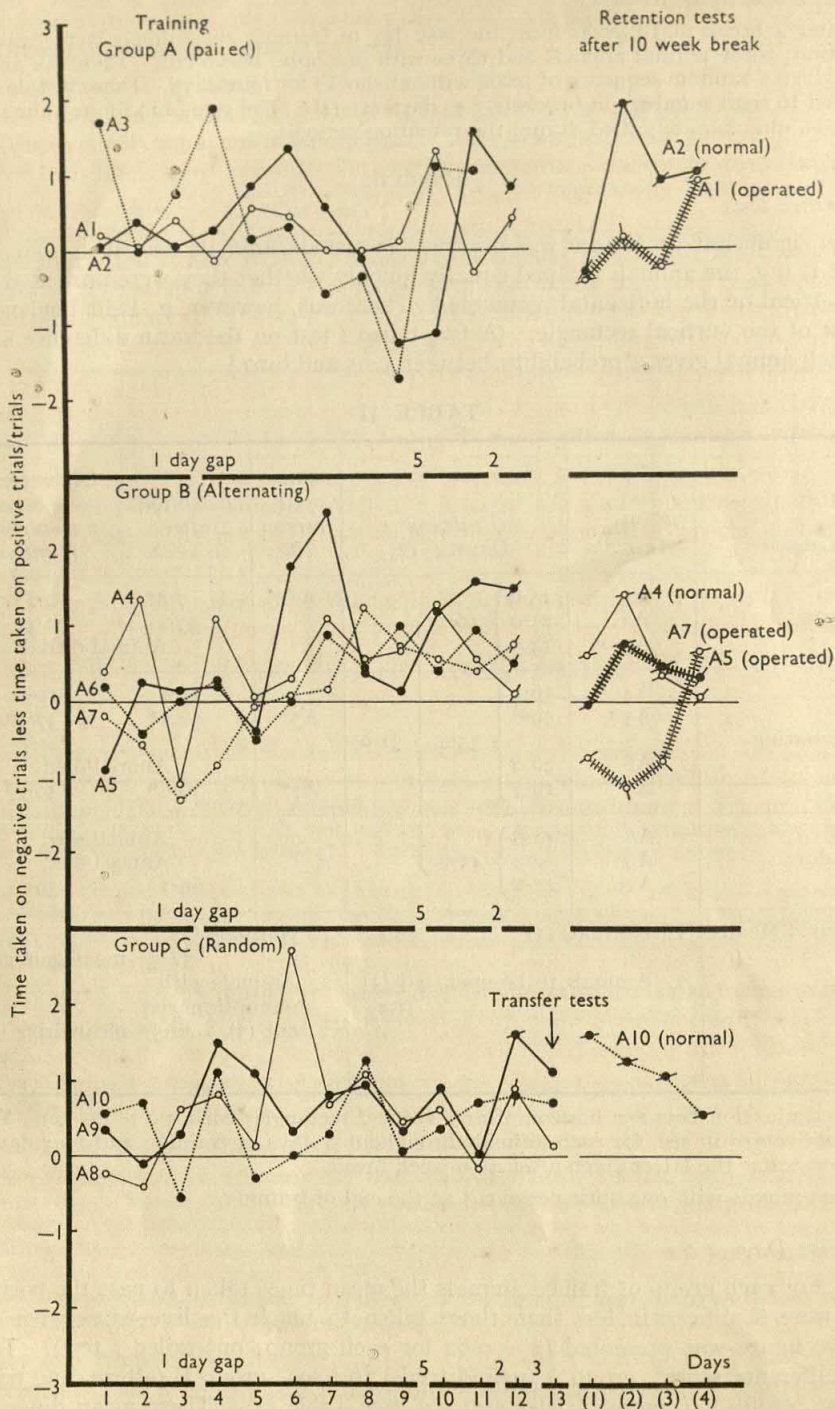


FIGURE 3

Mean difference scores (i.e. time taken on negative trials less that taken on positive trials) for individual animals belonging to the groups shown in Figure 2.

● = tests without shock.

===== = animals with one optic nerve severed immediately after training.

positive and negative figures (i.e. they still escape into the tank in spite of shocks—see Appendix) so very high difference scores would be unlikely to occur with this method of training.

2. It can be seen from Figure 3 that the pattern of learning in Group A is consistently different from that in B and C. Each animal in the paired trials group shows a marked drop in score over the period Days 7–9, whereas the animals of Groups B and C maintain a fairly steady positive score. An analysis of variance shows that although there is no significant difference between groups B (Alternating) and C (Random) there is a strong indication of a difference between the pattern of learning in Group A (Paired) and that in both Groups B and C together ($p = < 0.025$). Presumably it is easier for an animal to maintain a steady discrimination when the sequence of positive and negative trials is broken up (see Discussion).

3. The five-day break in training after Day 9 appeared to have little effect upon Groups B and C. With Group A, however, scores were considerably better for two of the three animals after the break (Fig. 3).

4. Tests, without shocks, given on Day 12 after a two-day break show that all the animals except animal A4 in Group B were capable of maintaining the discrimination learned without the reinforcement of shock on negative trials (Figs. 2 and 3).

The first three days of preliminary training have not been included in any statistical calculations as the fact that the animals were being encouraged across the wires on both positive and negative trials might impair the validity of the learning scores (see Appendix).

Transfer tests; Group C

The mean difference score for transfer to a rectangle 6.5×9.5 cm. was 39.0 sec. which compares favourably with the mean difference scores on Days 9, 10 and 11 for this Group (Fig. 2). Undoubtedly there is evidence of generalisation to a larger figure.

Retention tests after 10 weeks

1. The three normal animals showed either better or similar scores during retention tests than during training (Mean scores: retention 52.3 sec.; training 28.1 sec.). The three operated animals, however (one eye only) made either lower or negative scores during retention (mean scores: retention -0.3 sec. training 31.4 sec.). The mean differences in score made by the normal and the operated animals respectively were 24.2 sec. and -31.7 sec. These are significantly different between the probabilities of 0.05 and 0.025 (see Table II).

2. Apart from A1 and A7 (both operated) all animals showed some degrees of extinction after the first two days of tests (see Fig. 3).

DISCUSSION

The fact that toads can discriminate between a horizontal and a vertical rectangle of identical shape and area, on the basis of escape as reward and shock as punishment, confirms the earlier work of Pache (1952) and supports the behavioural observations of Franz (1927), Yerkes (1903) and Buytenijk (1918). According to the recent findings of Lettvin *et al.* two layers of cells in the optic tectum show a specific response to dark moving contours with straight edges. These layers relate to on-fibres (an unmyelinated group giving sustained contrast to a sharp edge moving *into* the visual

field) and off-fibres (a myelinated group responding to an edge moving through the visual field).

The level of discrimination maintained is clearly significant though the scores themselves are not high. The mean difference per trial (for all 10 animals) between the times taken to pass by the figure on positive trials and to touch the shock wires on negative trials is 31 sec. A greater difference is, however, hardly to be expected under the experimental conditions described. The animals are put into a position in which the two alternative actions are (1) immediate escape (positive figure); (2) delayed escape (negative figure). Supposing that the discrimination is in fact easy in relation to the toad's visual system the drive to escape, even when the negative figure is presented, may still be stronger than the drive to withdraw from mild punishment. Thus differences between time taken on the positive and negative trials would appear slight.

Although the learning of individual animals is erratic there is a clear growth in mean scores per group for the first eight to nine days (Figs. 2 and 3). After this the pattern of learning appears to be sufficiently well established for animals to repeat their performance accurately during tests without shock. Transfer to a larger figure supports the fact that learning is well established.

The normal animals showed good retention during tests without shock following a 10-week break in training. This points to the fact that, as already discussed, the strength of the representations set up in the memories of the animals is possibly stronger than the training scores indicate. Perhaps the shocks themselves have some inhibitory effect on learning. This retention, however, was not shown very clearly by the animals with one optic nerve cut after training. It must be presumed from this that binocular vision is important in the visual learning of the toad (Gaze, 1960), and, in view of the deliberate way toads focus their prey before snapping, this would not be altogether surprising.

It has been shown that with some mammals there is a marked tendency for animals to retain a visual task by learning the pattern of the sequence of trials if these are in non-random form (Sutherland, 1961). However, the groups of toads trained with a patterned sequence of trials did not learn significantly better than those trained with a random sequence. Evidently the amount of higher nervous tissue required for animals to learn the clues of a patterned arrangement needs to be greater than that required for the discrimination itself. It is important, however, that the recurrence of positive and negative stimuli should be broken up, i.e. continual pairs of consecutive figures do not allow for such good discrimination as figures that are randomized in such a way that the consecutive pairs only occur a limited number of times. During retention tests, however, with random trials all the normal animals showed high mean scores (Table II) so presumably differences in learning due to trial sequence are unlikely to be permanent.

APPENDIX A

Training Technique

The animals were first acclimatized to the experimental situation by being placed in groups of three to four under the direct glare of the artificial light and allowed to escape into the home tank. This procedure was carried out six times; three times with no figure at all and three times with the positive stimulus only.

1. During the first three days of full training certain techniques were devised in order to increase speed of response and initial learning:—

- (1) The animals were placed at 10.0, then 15.0 and finally 20.0 cm. from the edge of wires (see Fig. 1). This cut down the very high time scores for the first few days occasioned by placing untrained animals too near the back of the box.

- (2) A mild shock of 0.02 watts (40 volts, 0.0005 amps.) was given in order that the effect of the shock in the early stages was not so great as to hinder the animals from making any response at all.
- (3) After receiving a shock if an animal did not scramble across the wires to the tank it was left in the box until another attempt was made to cross. If the animal received a shock for the second time then it was removed from the box by the experimenter and placed in the tank.
- (4) On both positive and negative trials if an animal did not either escape or receive a shock after 5 min. it was gently encouraged to the alley way and pushed across the wires. It was observed that, in the early stages of the experiment, if an animal was removed as a reward on negative trials for staying at the back of the box, it would then tend to stay at the back of the box on all trials rather than make a decision. Without a time limit animals would stay back for as long as 10 min.

After the first three days and for the rest of the training these techniques were modified as follows:—

- (1) All animals were placed at 20.0 cm. or more from the grid.
- (2) On negative trials if an animal did not receive a shock by 5 min. it was removed from the box instead of being pushed through the alley way. This removal then constituted an additional reward.
- (3) On positive trials if an animal did not pass the stimulus by 5 min. it was gently pushed through the alley way.
- (4) If, after receiving a shock, an animal did not escape, it was not removed until it had turned away from the wires and crawled to within 10.0 cm. of the back of the box. (Thus more persistent animals sometimes received two or even three shocks before being removed.) It was hoped that this technique might encourage an animal to stay at the back of the box (see point 2).
- (5) The shock was strengthened to 0.03 watts (60 volts, 0.0005 amps.) now that the animals were accustomed to the situation. If escape were made too easy then differences between response times to positive and negative figures would probably be reduced.

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RETENTION OF SHAPE DISCRIMINATION AFTER REGENERATION OF THE OPTIC NERVES IN THE TOAD

BY

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Using a method of successive discrimination previously described by the author eight toads (*Bufo bufo*), were trained to discriminate first between horizontal and vertical rectangles, each 3.5×7.5 cm., and second between horizontal and vertical rectangles each 5.0×7.5 cm. Immediately after training, both optic nerves were severed in four animals and allowed to regenerate. After a period of 10 weeks without training both the operated and non-operated animals retained the capacity to discriminate both the large and the small rectangles at a level either not significantly different from training or significantly better than training.

INTRODUCTION

It is known that the optic nerve regenerates after section in urodeles, anurans and certain fishes (Gaze, 1960 review). Sperry (1951) put forward the hypothesis of neuronal specificity to explain the fact that toads, after regeneration of both optic nerves, show normal feeding responses. This theory, that the fibres of the optic nerve make synaptic contact in the appropriate region of the optic tectum, has been supported by the work of Gaze (1958), who showed by electrical recording from the optic tectum of *Xenopus* that normal point-to-point projection from the tectum to the retina is restored in animals with regenerated optic nerves. Lettvin *et al.* (1959, 1960) have also contributed to this work by showing that there are four functionally different cells layers in the tectum, each of which is reconstituted after regeneration.

In the present experiment the hypothesis has been tested that if toads are trained to distinguish between two shapes moving in the visual field they will retain this learning, after section and regeneration of both optic nerves, to the same extent as a similar group of animals not operated after training.

It has already been shown that toads can retain a learned discrimination between a horizontal and a vertical rectangle each 7.5×5.0 cm. over a period of 10 weeks but that perhaps some evidence of extinction is evident on the third day of testing (Parriss, 1963). In the present experiment it was decided to train animals primarily to discriminate between horizontal and vertical rectangles each 3.5×7.5 cm. as it was thought that the relatively greater proportion of length to width might make the task of discrimination easier than with the 7.5×5.0 cm. rectangles. As it appeared during training, however, that the learning of the small rectangles was somewhat more difficult for a number of the animals than the learning of the larger rectangles under similar conditions (Parriss, 1963), the animals in the present experiment were trained to discriminate between the 5.0×7.5 cm. rectangles *after* they had learned the 3.5×7.5 cm. rectangles. Thus it was hoped that animals would learn sufficiently well on one or other of the shapes to maintain retention over the period of regeneration.

METHOD

Animals

The animals trained were eight mature female toads (*Bufo bufo*) disturbed from hibernation in early March.

Training

A method of successive discrimination was used in which toads were trained to pass by a black moving figure in order to escape from an illuminated area into a dark tank with water and peat (Parriss, 1963). When the negative figure was presented they received shocks from a wire grid just below the figure; when the positive figure was presented they were allowed to escape without shock. The times taken for an animal (1) to place both forelegs on the grid during positive trials, (2) to touch the grid on negative trials, were scored. (Identical criteria were not used for recording positive and negative trials because during positive trials animals sometimes just touched the edge of the grid and then turned back into the box.) The difference between these times indicates the degree of discrimination present and is referred to as "difference score" or merely "score." Since it was not found possible to train animals to stay in the box on negative trials without their attempting to escape, the difference scores are not strikingly high. Once learning is apparent, mean difference scores may vary between 15 and 90 sec. per 10 trials.

The animals were given five positive trials and five negative trials per day. For convenience these were run alternately as it has been shown that toads do not learn better when trials are alternated than when they are random (Parriss, 1963).

Calculations

All calculations of significance have been based on the mean difference scores for individual animals.

Operations

The optic nerves were transected between the eye and the skull, incision being made through the roof of the mouth (Maturana, 1958). Where possible part of the dural sheath was left intact. Ether was used as anaesthetic.

Histology

After perfusion with formol saline the brains, with the posterior part of the optic nerve attached, were kept in 0.75 per cent. solution of formol saline for two months. They were then cut transversely at 10μ and stained with Holmes's method (1943).

Plan of experiment

A group of eight animals was trained to discriminate first between a horizontal and a vertical rectangle, both 3.5×7.5 cm. and then between a horizontal and vertical rectangle, 5.0×7.5 cm. In both cases the horizontal was the positive figure and the vertical was the negative. (It has been shown in a previous experiment that there is no significant preference for either of these figures without training (Parriss, 1963).) After 24 days of training the optic nerves were severed in four animals. After a period of 10 weeks, during which both operated and normal animals were kept under similar laboratory conditions without testing or training, the animals were given retention tests but no further training.

The training was divided into four sets of trials, marked on Table I as sets (a), (b), (c) and (d). Each set involved several days of continuous training. Between each set there was a gap of one week. In this way training was spread over a period of several weeks before the optic nerves were severed.

Four animals (group A), were given three sets of trials with the small rectangle and one with the large. The other four animals (group B), were given two sets of trials on the small figure and two on the large. Thus the total number of trials and sets of trials remained the same for each animal. At the end of the training period two animals were operated from each of these groups. The normal animals are marked A1, B1; the operated A2, B2 (see Table I).

RESULTS

Operations

By histological examination it was observed that both optic nerves of all the operated animals had been cut. There was a marked proliferation of glial cells in the regenerated nerves as compared with normal preparations (Joseph, 1954). No obvious differences were observed in the bundles of fibres projecting to the tectum.

TABLE I

MEAN DIFFERENCE SCORES IN SEC. FOR NORMAL AND OPERATED ANIMALS DURING TRAINING AND TESTING

	Training							
Groups	(A1) Normal		(A2) To be operated		(B1) Normal		(B2) To be operated	
Animals	1	2	3	4	5	6	7	8
Set (a) 7 days	Small Rectangle				Small Rectangle			
Individual means	2.5	8.2	34.7	36.1	19.0	- 6.3	41.4	25.6
Group means	5.4		35.4		6.4		33.5	
		20.4				19.9		
Set (b) 5 days								
Individual means	27.4	43.5	8.0	16.7	- 1.4	- 5.7	- 1.1	- 15.5
Group means	35.5		12.4		- 3.6		- 8.3	
		23.9				- 5.9		
Set (c) 4 days					Large Rectangle			
Individual means	26.1	21.4	14.3	20.9	- 17.5	5.4	- 4.0	50.9
Group means	23.8		17.6		- 6.1		23.5	
		20.7				8.7		
Set (d) 8 days	Large Rectangle							
Individual means	15.1	24.8	31.3	27.4	37.4	22.9	16.4	30.1
Group means	20.0		29.4		30.2		23.3	
		24.7				26.8		
Testing after 10-week retention period								
	Normal				Operated			
Set (e) 2 days					Small Rectangle			
Individual means	39.0	42.9	3.1	35.1	31.0	18.3	- 6.6	34.8
Group means	41.0		19.1		24.7		14.1	
Set (f) 2 days					Large Rectangle			
Individual means	32.1	4.7	10.7	36.8	4.5	32.7	32.1	40.5
Group means	18.4		23.8		18.6		36.3	

The horizontal and vertical rectangles were each 3.5 × 7.5 cm. (small) for the first part of the training and 5.0 × 7.5 cm. (large) for the latter part of the training.

Comparison of retention scores and training scores for normal and operated animals (Table II)

The scores shown by both operated and normal animals are either similar to or somewhat better than those shown during the training period. Combining the means for the four normal animals (groups A1, B1) and the four operated animals (groups A2, B2), the normal animals make a score of 33.8 sec. (small) and 18.5 sec. (large) on retention, as compared with 11.5 and 16.1 sec. on training; whereas the operated animals make scores of 16.6 sec. (small) and 30.0 sec. (large) on retention, compared with 17.1 and 26.4 sec. on training.

• The improvement of retention scores over training scores shown by the normal animals on the small figure is significant at the 0.01 per cent. level. There is no significant difference, however, in either a positive or negative direction, between training and retention scores for the operated animals nor for the normal animals on the large figure.

Training with the different sized rectangles

During training the eight animals in this experiment did not appear to discriminate between the 3.5×7.5 cm. rectangles as well as a comparable seven animals, described in a previous experiment, discriminated between the 7.5×5.0 cm. rectangles, but the difference is not significant.

There is also no significant difference between the learning of the small and the large rectangles for Group A or B, separately or together.

TABLE II

TOTAL MEAN DIFFERENCE SCORES IN SEC. FOR NORMAL AND OPERATED ANIMALS DURING TRAINING AND TESTING

Groups	Training			
	Normal A1 B1		To be Operated A2 B2	
Small Rectangle 	21.5	1.4	21.8	12.6
	11.5		17.1	
Large Rectangle 	20.0	12.1	29.4	23.4
	16.1		26.4	
	Testing after 10 weeks			
	Normal		Operated	
Small Rectangle 	41.0	24.7	19.1	14.1 ^c
	33.8		16.6	
Large Rectangle 	18.4	18.6	23.8	36.3
	18.5		30.0	

The horizontal and vertical rectangles were each 3.5×7.5 cm. (small) for the first part of the training and 5.0×7.5 cm. (large) for the latter part of the training.

Extinction

There is no clear pattern of extinction for either the normal or operated animals throughout the four days of tests. Taken as a group, however, the operated animals show the best discrimination on the fourth day of tests whereas the normal animals show the best discrimination on the second day. (Mean score normal animals—Day 1, 18 sec.; Day 2, 49 sec.; Day 3, 22 sec.; Day 4, 15 sec. Mean score operated animals—Day 1, 9 sec.; Day 2, 24 sec.; Day 3, 17 sec.; Day 4, 44 sec.). It is possible that differences in retention between the normal and the operated animals could be partly related to different patterns of extinction (see Discussion).

DISCUSSION

Sperry (1951) has already shown that toads are capable of making normal feeding responses to prey after section and regeneration of both optic nerves, and the recent

work of Gaze (1958) and Lettvin *et al.* (1959) shows that this is indeed due, as Sperry has suggested, to point-to-point reconnections between the retina and the optic tectum. The present results show that this reconstitution extends to the preservation of a learned response after regeneration of the optic nerves. Experiments carried out by Sperry with the small fish, *Gambusia Holbrooki*, show retention of a colour discrimination after regeneration but there appears to be no previous example of retention of shape discrimination.

The normal animals in the present experiment retained the first discrimination significantly better than the operated animals. They also appeared to show extinction more quickly than the operated animals. The interpretation of this phenomena is uncertain but it is possible that the normal animals would in fact retain the second discrimination also better than the operated animals if rates of extinction were equal for both groups. However the difference in retention between the normal and operated animals suggests that although each part of the retina connects with a specific area of the optic lobe the reconstitution of the original nerve fibre pattern may not be entirely precise.

Sperry points out that the axons of the growing nerve spread widely in the tectum and suggest that there may be numerous branches which fail to establish appropriate connection and therefore undergo atrophy and resorption. According to his theory each ganglion cell of the retina, together with its optic nerve fibre, possesses unique biochemical properties to which each neuron of the optic lobe is specific.

Although the exact nature of this synaptic association between the retinal cells and those of the optic tectum is not yet known, it is safe to assume that the representations set up in the brain must largely be stored in the cells of the tectum. Only further experiments can determine the influence of the cerebral hemispheres of the brain on the storing of these representations (Warren, 1961).

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SERIAL ROTE-LEARNING: INCREMENTAL OR ALL-OR-NONE?*

BY

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If the associative connections in a serial list are acquired in an all-or-none fashion, rather than gradually with every trial adding an increment of associative strength, then changing the serial order of the middle items in the list during the course of practice should have no effect on the rate of learning the list as a whole or even of the particular items that have been interchanged. Thirty subjects learned a serial list by the anticipation method. The middle items of the list were reversed in serial order approximately half-way through the number of trials required for mastery. The subjects took no longer to learn the list and made no more errors than did 30 control subjects for whom there was no change in serial order. The serial-position curves of the two groups were almost identical. It was also shown that the learning "curves" of single items in the series, when plotted for individual subjects do not reveal a gradually increasing probability of the correct response, but show instead a sudden jump on one trial from the chance guessing level to a level close to 100 per cent. correct responses. The results are consistent with a non-incremental theory of serial learning.

INTRODUCTION

Using the method of paired-associates, Rock (1957) found in two experiments that pairs presented but once before learning were learned as rapidly as those repeated until they were learned. Replicating Rock's experiments with additional controls, Clark, Lansford, and Dallenbach (1960) corroborated Rock's results. It appears from these experiments that repetition plays no role in the formation of associations other than that of providing the occasion for new associations to be formed on a single trial. Within the framework of the prevailing theories of learning stemming from Ebbinghaus, Pavlov, Thorndike, and Hull, this is a radical hypothesis that demands further investigation. Indeed, the idea that learning is a gradual process, with each repetition or reinforcement of an S-R event adding an increment of associative strength, has been, until Rock's findings, one of the least questioned assumptions underlying S-R conceptions of the nature of learning.

The present experiment was intended to test the hypothesis, originally suggested by Bolles (1959) that a subject, in learning a serial list by the anticipation method, does not acquire an increment of associative strength for all the S-R associations in the series with every trial. The usual theoretical explanations of serial learning phenomena, such as Hull's theory (Hull *et al.*, 1940), assume that the associative strength between the items in the series increases by gradual increments on every trial and that the greater difficulty in learning the middle items, as evinced by the bow-shaped serial-position curve, is the result of inhibition that accumulates in the middle of the list. An alternative to this traditional "incremental" or "continuity" theory is the notion that the subject learns each item in the series in an all-or-none fashion on a single trial, while on that particular trial the as yet not learned items do not gain an iota in associative strength. The serial-position effect would then be attributable simply to the order in which the subject learns the items in this all-or-none fashion. The beginning and end of the list generally are learned first and the

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middle items are learned last. Thus, according to this view, the S-R connection in the middle of the list should not acquire any associative strength until late in learning, so that even if the order of these middle items were reversed during the early learning trials, nothing should be lost and the subject should learn the serial list in the same number of trials as if there had been no change in the serial order.

The only experimental tests of this prediction were performed by Bolles (1959), who found that interchanging the fourth and sixth items in a nine-item list one-third of the way through learning had no significant effect on mean trials for learning or on the shape of the serial-position curve, as compared with the control conditions in which there was no change in serial order. Bolles concluded that serial learning follows non-continuity principles and that the associative strength necessary for performance is acquired in one trial.

An important theoretical point such as this one indeed requires more than a single experimental test, especially in this case, since Bolles's experiments and their analysis leave something to be desired in terms of experimental procedure. Bolles's two experiments used only eight and six subjects, respectively, and the window of the memory drum had to be moved in the course of learning to reveal the changed list, a procedure that could have alerted subjects to the changed conditions. That the same manipulation prevailed in the control condition is not an adequate safeguard.

The present experiment was designed as a more stringent test of the non-continuity hypothesis with regard to serial learning. Since learning a list of nonsense syllables consists of two phases—(1) learning the items that compose the list, and (2) learning their serial order—and since here we wish to test the non-continuity hypothesis only with respect to the *serial* learning phase, this experiment used, instead of nonsense syllables, coloured geometric forms with which the subjects were made familiar before having to learn their serial order. The interchange of the fifth and sixth items in a 10-item series occurred without any manipulation of the apparatus or any delay between trials. The interchange occurred on the same trial for all subjects in the experimental group. The change trial (Trial 7) was set, on the basis of previous tests with similar subjects, at approximately the average half-way point in learning. A constant serial order was used for all subjects, so that any irregularities in the shape of the serial position curve for one particular order of stimuli would not be "averaged out"; if learning was not incremental, the experimental condition should have no effect and the serial-position curves of experimental and control groups would be expected to manifest the same general irregularities. Also, the data of the present experiment are based on a much larger sample of subjects than Bolles used, and they were subjected to a more detailed, trial-by-trial, statistical analysis.

METHOD

Subjects. Sixty-three students were recruited from an introductory course in educational psychology. All were naive to serial learning experiments. The records of three subjects were eliminated from the analysis: one subject in the Experimental Group was emotionally upset by the task and quit before attaining the criterion of mastery; another subject in the Experimental Group mastered the entire list before the change trial; and one subject in the Control Group, having failed to show any appreciable learning in 40 trials, was dismissed. The Experimental and Control Groups each had 30 subjects.

Apparatus. The stimuli, consisting of triangles (T), squares (S), and circles (C), coloured red (R), yellow (Y), blue (B), and white (W) were projected on a screen by an automatic device which has been described in detail elsewhere (Jensen *et al.*, 1962). The subject sat approximately 3 ft. from the screen. The stimuli, $2\frac{1}{2}$ in. in size on the screen and of vivid colour, were projected at a 3-sec. rate with a 6-sec. intertrial interval. The series was always preceded by a green light that appeared at the bottom of the screen as the signal for the subject to anticipate the first item in the series.

Procedure. All subjects were instructed that they would have to learn, by the anticipation method, the serial order of ten different coloured forms, consisting of triangles, squares, and circles coloured red, yellow, blue, and white. They were informed that no item would be repeated in the series and that the same colour or shape would never be adjacent in the series. These instructions are such that subjects are able to name all the stimuli in the series before the first presentation, so that they could be required to make their first anticipations by sheer guessing on the very first trial. Subjects responded by saying "red square," "white circle," etc. This procedure minimizes the response learning phase of the task so that practically all the subject has to learn is the *serial order* of the items. The aim of the experiment was never mentioned to the subjects and, of course, there was no hint in any respect of the procedure that the serial order was to be changed in the course of learning for half the subjects. All subjects were required to learn the list to the criterion of mastery, i.e. one trial with correct anticipation of every item.

Control condition. The serial order for the Control Group throughout all trials was: (green light), RS, WC, YT, BS, YC, RT, WS, RC, WT, BC.

Experimental condition. For the first six trials the serial order was: (green light), RS, WC, YT, BS, RT, YC, WS, RC, WT, BC. On Trial 7 Items 5 and 6 were interchanged, so that the order for all succeeding trials was exactly the same as the Control condition, thus: (green light), RS, WC, YT, BS, YC, RT, WS, RC, WT, BC.

It should be pointed out that to test the hypothesis under consideration does not require any more complex design than that employed here. Nothing would be gained, for example, by counterbalancing the order of the items in Positions 5 and 6 in another pair of Experimental and Control groups. Since both groups in the present design learn the same serial order after Trial 6, it does not matter if the ease of learning the sequence RT-YC is different from that of YC-RT. In any case, according to the incremental theory of learning, the Experimental Group after Trial 6 should be at a disadvantage as compared with the Control Group.

RESULTS

In brief, there were no detectable statistically significant differences between the performances of the Experimental and Control Groups. The data were analysed separately for the Before Change and After Change trials. Table I presents the mean trials, errors, and per cent. errors and their standard deviations (SD) in both stages of learning. Contrary to the prediction from continuity theory, the Control Group actually took slightly more trials to learn and made more errors than did the Experimental Group. The mean errors in Positions 5 + 6, i.e. the interchanged items, were

TABLE I

MEAN NUMBER OF TRIALS, ERRORS, AND PERCENTAGE OF ERRORS* BEFORE AND AFTER SERIAL-ORDER CHANGE

	Before change: Trials 1-6			After change: Trials 7-Criterion		
	Experimental	Control	Difference	Experimental	Control	Difference
Mean trials ..	(6)	(6)	—	9.20	9.33	- 0.13
SD ..	(0)	(0)	—	4.63	4.30	—
Mean errors ..	37.33	38.20	- 0.87	32.93	33.03	- 0.10
SD ..	7.17	6.47	—	20.57	16.47	—
Mean per cent.						
Errors ..	62.23	63.68	- 1.45	33.93	35.44	- 1.51
SD ..	11.95	10.79	—	9.63	11.80	—

* Percentage of errors = $\frac{100 \times \text{incorrect responses}}{\text{correct} + \text{incorrect responses}}$

19.5 and 19.9 for the Experimental and Control Groups, respectively. All of the differences between the Experimental and Control Group, both in means and *SDs*, are minute and statistically non-significant.

It should be noted that the total number of trials required to attain mastery was, on the average, approximately 15 trials. Thus, the serial order change on Trial 7 came, on the average, slightly less than half-way to the criterion of mastery.

TABLE II
LEARNING CURVE AT SERIAL POSITION 5

Trial	Number correct		Number of subjects		Per cent. correct		χ^2 *	$P \geq$
	Experi- mental	Control	Experi- mental	Control	Experi- mental	Control		
1	2	1	30	30	6.67	3.33	0.00	1.00
2	5	3	30	30	16.67	10.00	0.14	0.69
3	5	2	30	30	16.67	6.67	0.65	0.42
4	8	3	30	30	26.67	10.00	1.78	0.19
5	11	4	30	30	36.67	13.33	3.20	0.07
6	2	7	30	30	6.67	23.33	2.09	0.16
7	5	4	30	30	16.67	13.33	0.00	1.00
8	5	6	28	29	17.86	20.69	0.08	0.76
9	7	3	27	29	25.93	10.43	1.37	0.25*
10	7	13	27	29	25.93	44.83	1.43	0.23
11	5	7	27	29	18.52	24.14	0.35	0.55
12	8	10	25	26	32.00	38.47	0.04	0.92
13	10	11	22	25	45.45	44.00	0.00	1.00
14	5	10	20	22	25.00	45.45	1.12	0.31
15	10	12	16	20	62.50	60.00	0.00	1.00
16	7	12	11	16	63.64	75.00	0.04	0.84
17	3	9	9	11	33.33	81.32	**	< 0.10
18	5	5	7	9	71.43	55.56	—	n.s.
19	5	5	6	8	83.33	62.50	—	n.s.
20	2	3	5	6	40.00	50.00	—	n.s.
21	1	2	3	6	33.33	33.33	—	n.s.
22	0	1	3	4	0.00	25.00	—	n.s.
23	1	1	3	2	33.33	50.00	—	n.s.
24	1	1	2	2	50.00	50.00	—	n.s.
25	0	1	1	1	0.00	1.00	—	n.s.
26	1	—	1	0	100.00	—	—	—
27	0	—	1	0	0.00	—	—	—
28	0	—	1	0	0.00	—	—	—

* Chi square with Yates's correction.

** Beyond this point Fisher's exact probability test was used (Siegel, 1956, pp. 96-100). Probabilities greater than 0.10 are indicated as non-significant (n.s.).

The serial-position curves of the Experimental and Control Groups are almost indistinguishable. This is true also when the curves are plotted separately for Trials 1-6 and for the trials after the reversal (7-criterion). The intraclass correlation (Haggard, 1958) may be regarded as an index, ranging from - 1.00 to + 1.00, of the degree of similarity between the serial-position curves of the Experimental and Control Groups. The intraclass correlation between the curves based on the trials before the change is 0.97; for the trials after the change it is 0.99.

Tables II and III present the "learning curves" for the reversed positions (5 and 6) for each group throughout the entire course of learning. The point of serial reversal is

indicated by the line after Trial 6. The significance of the difference between the groups in per cent. of correct responses on each trial was tested by chi-square up to Trial 17. In the trials from 17 on, the frequencies were too small for the valid use of chi-square and the Fisher exact probability test was used (Siegel, 1956, pp. 96-101). The two-tailed probabilities of the obtained chi-squares (taken from Table VI in Kelley, 1948, p. 202) reveal no significant differences (taking the traditional $p < 0.05$

TABLE III
LEARNING CURVE AT SERIAL POSITION 6

Trial	Number correct		Number of subjects		Per cent. correct		χ^2 *	P $\geq$
	Experi- mental	Control	Experi- mental	Control	Experi- mental	Control		
1	5	3	30	30	16.67	10.00	0.14	0.69
2	4	7	30	30	13.33	23.33	0.45	0.48
3	4	7	30	30	13.33	23.33	0.45	0.48
4	8	6	30	30	26.67	20.00	0.09	0.76
5	9	11	30	30	30.00	36.67	0.08	0.76
6	8	15	30	30	26.67	50.00	2.54	0.11
7	10	8	30	30	33.33	26.67	1.07	0.31
8	17	12	28	29	60.72	41.38	1.43	0.23
9	15	15	27	29	55.56	51.73	0.00	1.00
10	14	17	27	29	51.86	58.63	0.06	0.84
11	19	19	27	29	70.38	65.52	0.01	0.92
12	17	16	25	26	68.00	61.54	0.04	0.84
13	13	18	22	25	59.10	72.00	0.39	0.54
14	12	16	20	22	60.00	72.73	0.30	0.61
15	11	18	16	20	68.75	90.00	1.39	0.23
16	6	14	11	16	54.55	87.50	2.17	0.13
17	6	10	9	11	66.67	90.91	**	n.s.
18	6	8	7	9	85.72	88.89	—	n.s.
19	6	3	6	8	100.00	37.50	—	n.s.
20	3	5	5	6	60.00	83.33	—	n.s.
21	3	2	3	6	100.00	33.33	—	n.s.
22	2	3	3	4	66.67	75.00	—	n.s.
23	2	1	3	2	66.67	50.00	—	n.s.
24	1	1	2	2	50.00	50.00	—	n.s.
25	0	0	1	1	0.00	0.00	—	n.s.
26	0	—	1	0	0.00	—	—	—
27	1	—	1	0	100.00	—	—	—
28	0	—	1	0	0.00	—	—	—

* Chi square with Yates's correction.

** Beyond this point Fisher's exact probability test was used (Siegel, 1956, pp. 96-100). Probabilities greater than 0.10 are indicated as non-significant (n.s.).

as "significant") between the two groups on any trial. Also, a chi-square test of the difference between the groups in going from Trial 6 to Trial 7 (the change trial) revealed no significant difference at either Position 5 or 6.

Only one of the 30 subjects in the Experimental Group expressed any doubt at the end of the experiment that the serial order had remained constant throughout all the trials. This subject attained criterion in only eight trials and remarked to the experimenter that she had the feeling that the list had changed at some time, but she was not certain. Subjects were never directly questioned on this matter, since the

object of the experiment, of course, had to be kept secret. However, several psychologically sophisticated subjects (research assistants and one professor), who were not included in the experiment proper, were tested and questioned about the change. None was at all aware of the change and all expressed surprise when informed of it.

Supplementary data

If learning the serial position of each item takes place on a single trial for each item, as suggested by the present experiment, the "learning curve" for single items theoretically should show a sudden jump from a probability of zero for making a

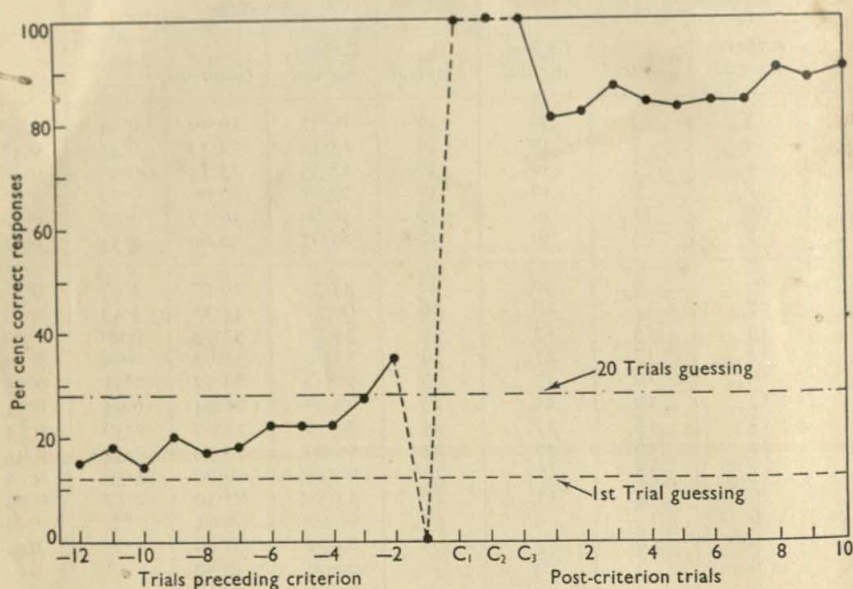


FIGURE 1

A backward learning curve, showing the mean percentage of correct responses on each item in the serial list among a group of 60 subjects on the 12 trials preceding the criterion of three successive correct trials.

correct response to 100 per cent. correct responses. In learning the serial order of items that are already familiar to the subject, as in the present experiment, the learning curve should jump from the chance guessing level to approximately 100 per cent. correct responses, but it will always be slightly less than 100 per cent. since some subjects will attain the criterion of learning merely by chance guessing.

To determine the probability of making a correct response as a function of number of learning trials, further data were obtained using the same form of serial learning as in the previous experiment. The subjects (60 university students) learned by the anticipation method a nine-item serial list composed of triangles, squares, and circles coloured red, yellow, and blue. The procedure was approximately the same as for the previous experiment. Subjects were informed that items of the same shape or colour would never be adjacent in the series. The stimuli were projected one at a time on a screen at a 3-sec. rate with a 6-sec. intertrial interval. All subjects learned to a criterion of three successive perfect trials.

* A backward learning curve (Hayes, 1953) was obtained for each item in the list. These "curves" are plotted backward from the trial on which the subject attained the criterion, which consisted of anticipating the item correctly on three successive trials. The backward curves were then averaged for all items. The resultant mean curve is shown in Figure 1, which represents the percentage of correct responses made by all subjects on the 12 trials preceding the criterion. The -1 Trial shows the last error made before attaining the criterion of three successive correct responses (C_1 , C_2 , C_3).

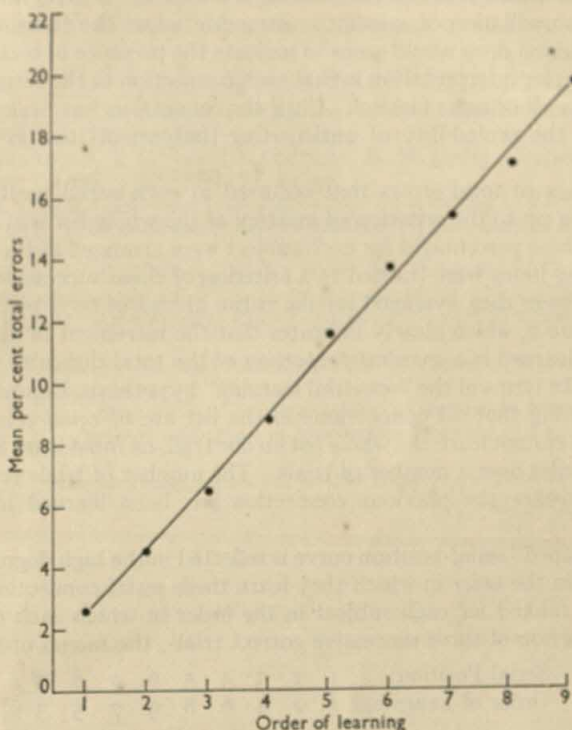


FIGURE 2

Mean percentage of errors on each item plotted in the order (determined for each subject) in which each item in the serial list was learned to a criterion of three successive correct trials. ($N = 60$).

The post-criterion trials show the usual drop. The "20 trials guessing" line indicates the mean percentage of correct responses made by a control group of 20 subjects who were presented with a different serial order of the items on each of twenty trials and for whom every anticipation was necessarily a sheer guess. These subjects knew the items composing the lists and they knew the rules that adjacent items would never be of the same shape or colour. The "first trial guessing" indicates the mean percentage of correct responses made by all subjects on the first trial, which consisted of sheer guessing. This level is lower than the mean for guessing 20 trials because of a greater number of omissions on the first trial. Only at the -2 trial did the percentage correct rise above the level of chance guessing. This may have been because for some subjects an association was actually learned the first or second time the item was anticipated correctly and the criterion of three correct responses was too stringent.

This criterion of learning seemed necessary, however, because of the relatively high chance probability of guessing a correct response. The important point is that there is not a significant increase above a chance level in the probability of a correct response on the several trials preceding the criterion.

This phenomenon brings into consideration two different theoretical interpretations. If the learning is a gradual process, with every trial adding an increment to the associative strength for the correct response, there must be a performance threshold which is attained before the learning is manifest. Furthermore, there must be practically no oscillation of associative strength before the threshold is attained. Yet the post-criterion drop would seem to indicate the presence of oscillation. What seems to be a simpler interpretation is that each connection in the serial list is learned on one trial in an all-or-none fashion. Until the connection has been learned on one particular trial, the probability of anticipating the correct item is not above the chance level.

The percentage of total errors that occurred at each serial position during the course of learning up to the criterion of mastery of the whole list was determined for each subject. These percentages for each subject were arranged in the order in which the corresponding items were learned to a criterion of three successive correct trials; the percentages were then averaged for the entire groups of 60 subjects. The result is shown in Figure 2, which clearly indicates that the increment in difficulty in each successive item learned is a *constant proportion* of the total difficulty of learning the list as a whole. In terms of the "one-trial learning" hypothesis, Figure 2 can be interpreted as indicating that all connections in the list are of *equal* relative difficulty. Since the subject cannot learn the whole list on one trial, he must learn the connections in a particular order over a number of trials. The number of trials required to learn each connection once the previous connection has been learned is equal for all connections.

The "bow-shaped" serial-position curve is reflected in the high degree of agreement among subjects in the *order* in which they learn these serial connections. When the connections are ranked for each subject in the order in which each connection was learned to a criterion of three successive correct trials, the modal order is:—

Serial Position:	1	2	3	4	5	6	7	8	9
Order of Learning:	1	2	4	6	8	9	7	5	3

As a measure of agreement among subjects in the order of learning the connections, Kendall's coefficient of concordance W was computed and was found to be 0.69. The cause of this high degree of unanimity in the order of learning is not yet known.

DISCUSSION

The results of the present experiments are essentially the same as those found by Bolles (1959). But these results, taken by themselves, are only *consistent* with a non-continuity theory of serial learning. They do not *prove* the theory. Hull's continuity-type theory, which explains the greater difficulty in learning the middle of the list as a result of the accumulation of inhibitory tendencies, is capable of interpreting the results of the present experiment and of Bolles's experiment. If inhibition gradually builds up on the middle S-R connection (Positions 5-6), which would normally oppose the subject's correct performance on these items until late in learning when the associative strength has grown sufficiently to overcome the inhibitory tendency, and then, if, in the course of practice, a new S-R connection (say, 6-5) were put into the list to replace the connection that had already accumulated inhibitory tendencies, the inhibition attached to the original S-R connection would be diminished or eliminated.

Since a part of the series would already have been learned, not so much inhibition would accrue to the new S-R connection and the remaining task would be as easy or possibly even easier, to learn as if the list had remained unchanged throughout all the trials. But this is mere wishful thinking along Hullian lines, for there is no independent support of Hull's notion that there is a greater inhibitory potential in the middle of the series. Thus, the results of the present experiments would seem to further weaken the continuity of incremental hypothesis and lend support to the all-or-none theory of serial learning. The writer has discussed elsewhere (Jensen, 1962), the broad implications of these findings for a theory of serial learning.

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SLIPPAGE OF CONTACT LENSES AND OTHER ARTEFACTS IN RELATION TO FADING AND REGENERATION OF SUPPOSEDLY STABLE RETINAL IMAGES

BY

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After-image experiments reveal that scleral-fitting contact lenses slip through several minutes of arc. Experiments in which they have been used to procure "stabilized" retinal images must therefore be reinterpreted. Light-weight, suck-on, limbal-seating cups are much better, but complete stabilization cannot be relied upon. Other possible artefacts in stabilized image work are described and are thought to be responsible for certain features of previous accounts of the appearance of stabilized images. Discounting features caused by artefacts, the image appears to blur, losing detail, and fade, losing contrast, within the first few seconds. Contrast and detail are not regained, but the milky, textureless image that remains fluctuates in appearance for a minute or so. Finally the fluctuations die out and a very blurred, cloudy version of the original scene persists.

During the last few years Ditchburn and his group in this country, Riggs and his colleagues in the U.S.A. and Yarbus in the U.S.S.R., have all described methods intended to hold an image constantly in one position on the human retina: references to representative papers are given in the list at the end of this paper. The results obtained by the different groups of workers are not in good agreement with each other, and it is therefore helpful first to refer to the background of information available as to what happens when a pattern of light is held unchanged on the retina.

The following are well-known entoptic images:—the shadows of the retinal vessels produced by trans-scleral illumination: the shadows of the retinal capillaries brought into view by moving a small pinhole in front of the pupil while looking at a uniformly illuminated surface: Maxwell's spot: Haidinger's brushes. A detailed description will not be given since full accounts are available in standard works. They all require some special manoeuvre to make them detectable, in spite of the fact that the physical characteristics corresponding to the image are in some cases constant features of the light striking the receptors. But what is significant from the present point of view is that when they are thus brought out, they remain visible only for a very short time. They fade, and some further change is required to make them visible again. This implies that, in the case of the eye, a steady state of physical excitation yields only a very transient perception, and there are two other phenomena which fit in with this implication. First, if voluntary fixation is very carefully maintained for a period of half a minute or so, then the visual field appears to fade, losing contrast first in the periphery, later towards the foveal region. Second, after-images characteristically fade from view very rapidly, and if they are to be seen with anything like their original clarity, the eyes must be opened and closed, or the illumination of the retina changed in some other way. Furthermore the very rapid adaptation of the nervous discharge shown by retinal ganglion cells provides a reasonable physiological basis for fading of this sort, though one must obviously be cautious in relating impulse frequency to sensation intensity, and in extrapolating from animals to man.

This background of knowledge leads to the clear expectation that any arbitrary pattern of light held stationary on the retina will fade and cease to be visible within

a few seconds of first presentation, but only Yarbus's results fit in with this. Ditchburn and Ginsborg (1952) first reported that a divided field 1° in diameter, with one half illuminated, the other half black, disappeared "for a few seconds every minute." Later Ditchburn and Fender (1955) reported that with "improved stabilization" a 4-minute line was visible for "only 50 per cent of the time." Even in later work, this group has not reported anything that disappeared from view for more than 90 per cent. of the time. Riggs, Ratliff, Cornsweet and Cornsweet (1953) stabilized the image of dark lines dividing a 36 minute field. They reported the expected fading of the image, but they agree with Ditchburn and colleagues that the image usually reappears; they noted, however, that the finest lines were seen clearly at first, but then disappeared permanently and completely. In contrast to these reports Yarbus (1957*a, b*) says that a stabilized part of the visual field takes on its characteristic "empty" appearance within 3 sec. of presentation, and does not change thereafter if nothing else is changed.

The obvious disagreements between Ditchburn and Rigg's groups on the one hand, and Yarbus and the expectations from entoptic stable images on the other, would clearly be explained if Ditchburn and Riggs achieved less complete stabilization than they believed. Their technique uses a contact lens which seats on the conjunctiva; this is firmly attached to the eye only at the limbus, and elsewhere loosely overlies the eyeball. The only evidence that such a contact lens accurately follows the movements of the eye is that records of its movement are of a very similar pattern to those obtained (not simultaneously) from a mirror placed on the cornea (Ratliff and Riggs, 1950), and that changing the inertia of the contact lens has little influence on the pattern of records obtained (Riggs, Armington and Ratliff, 1954). Clearly one would like better evidence, and the main object of this paper is to describe a method of detecting displacements over the retina of supposedly stable images, and sample results obtained with various patterns of lens. These results show that it is very difficult to avoid slippage and consequent displacement of the image. During the course of the work many artefacts, which may account for the lack of agreement of previous workers, have been encountered, and these will be briefly described. An account will be given of the subjective appearance of images stabilized as well as possible.

METHODS

Contact lenses. The full-fitting type (Fig. 1 (*a*)) were made by Messrs. Keeler's of London. They were instructed to make the type ordinarily used for stabilized image work, with a close fit at the limbus. This is important, since the conjunctiva upon which they seat is attached to the sclera only at the limbus, whilst pressure on the limbus is sedulously avoided in making contact lenses for ordinary use, owing to the discomfort it is liable to cause over long periods of wear. An artificial pupil was painted on the lens, and a supplementary lens of + 40 D moulded in the perspex. To prevent smearing of the lens a low aluminium tube was glued round the pupil, and it was convenient to continue one side of this out as a girder holding the target at 25 mm. from the eye.

Yarbus type suck-on lenses. These were made by spinning aluminium sheet (0.004 in.) on to a form of the appropriate shape (Fig. 1 (*b*)). This was pierced and a rubber sucker glued on. The lenses were attached in front of the 2 mm. hole, and the targets were held 15 to 25 mm. from the eye on a light aluminium girder. They were filled with 1.5 per cent. NaHCO_3 , instead of air. These lenses weighed about 350 mg. complete with target and fluid. This is a great deal less than the full-fitting type, but Yarbus claims that his weigh even less (100 mg.). The optical quality of the image is, however, better than we have achieved by following Yarbus's methods more closely.

These lenses can also be made by pushing a steel ball 15 mm. in diameter heated to 200°C . into thin perspex sheet. Such lenses tend to be more robust, but heavier, than those spun from aluminium.

Short-focus lenses. Short-focus lenses for mounting on the aluminium caps are conveniently made from spherical melted glass beads ground and polished to a flat surface on one side. The convex surface lies in the tear fluid.

After-image apparatus. The arc of a high-pressure mercury vapour light (G.E.C. 250-watt type M/ED) is focused by a wide aperture lens onto the place to be occupied by the artificial pupil of the contact lens. This is seen in Maxwellian view, but defocused since the lens lies further from the eye than the target of the contact lens. A photographic shutter is placed in the light path, and in certain experiments the light also passes through a polaroid filter.

Photographic flash bulbs held just beyond the target of the contact lens can also be used.

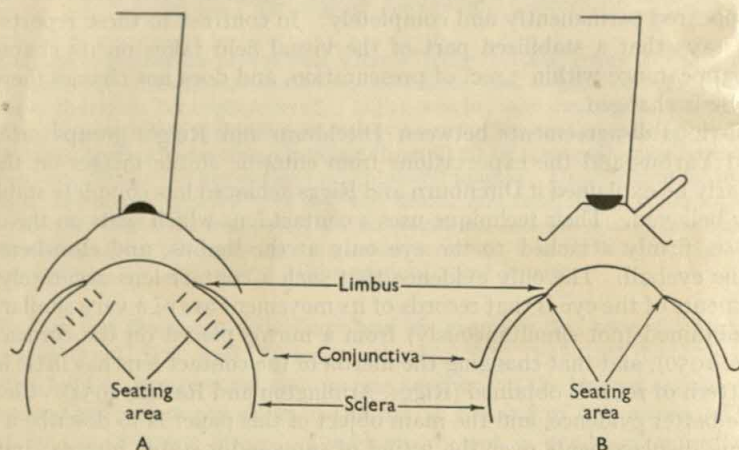


FIGURE 1

Contact lenses of full-fitting (A) and Yarbush-type (B) for stabilized images, showing that A seats mainly on the conjunctiva where it loosely overlies the sclera, whereas B seats near the attachment of conjunctiva to sclera at the limbus.

Local anaesthetic. Amethocaine was used to anaesthetize the cornea when using suck-on lenses. If it is not used, a mildly unpleasant sensation occurs when a Yarbush type lens seats near the limbus, and this increases to distinct pain when the suction is applied. This would be quite easily bearable, but accidental jarring of the lens, and attempts to blink, are exceedingly painful and necessitate the use of anaesthetics. Perspex lenses with smooth edges and fitting the cornea and limbal region quite closely are less painful than the Yarbush type, but suction and accidental jarring are still so unpleasant that anaesthetics are needed.

The full-fitting type does not need an anaesthetic.

Injuries. Corneal abrasions have resulted from accidental jolting of suck-on lenses, particularly those having sharp edges. They heal rapidly. Albucid has been used to minimize risk of infection when it was thought that abrasions might have occurred.

RESULTS

The results are given in four sections, on contact lens displacement, on other artefacts, on the appearance of stabilized images, and on controls and miscellaneous observations.

Contact Lens Displacement

Qualitative detection. It is a reasonable assumption that the events causing an after-image occur in the retina, and therefore cannot move over it. In view of the evidence produced by Craik (1940) and Brindley (1959) as to their causation, and the

satisfactory results in agreement with objective measures that have previously been obtained by using them to measure eye-movements (e.g. Barlow, 1952), the onus of disproof must rest on anyone who doubts this assumption. There should therefore be no movement of a stabilized image relative to an after-image so long as stabilization is good, but if it is not then there will be relative movement between the two and it should be possible to perceive this movement. This is the principle of the first method of detecting slip. The target shown in Figure 2 (a) is first presented and a bright flash of light lasting $1/25$ sec. is delivered. The black card with a notch is then removed from the end of the target-bearing girder of the contact lens, revealing the target shown in Figure 2 (b). The dotted outline shows the position formerly occupied by the notched card, and hence the position in the visual field occupied by its after-image. By varying the background light behind the target it is possible for a minute or longer to see for brief instants both the "stabilized" pointer, and the after-image.

Using the full-fitting contact lenses, relative movement is unmistakably detectable; the quantitative extent is variable, depending on the movements executed by the eye,

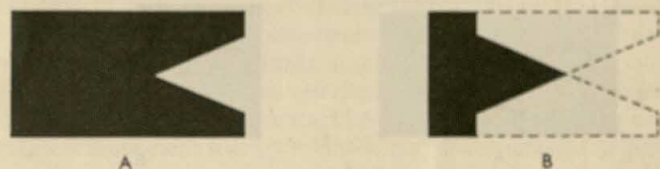


FIGURE 2

Qualitative method for detecting incomplete stabilization. (a) The target used when the very intense flash of light is delivered. (b) The target after the notched card has been slipped off, showing the pointer (which is stationary relative to the contact lens), and the dotted outline of the after-image (which is stationary relative to the eye). Relative movements of the two are observed.

but even when voluntary fixation is attempted it is hard to avoid small movements of the two pointers relative to each other. When a voluntary saccadic movement of a few degrees is made, then relative movement shows up indicating that the "stabilized" image does not move as far as the after-image.

If rather bright lights have been used for rendering after-image and stabilized image perceptible, then when the contact lens is removed a somewhat blurry after-image of the pointer may be detectable. If the original after-image is still present one has an opportunity to inspect the two after-images in the same way that one previously inspected an after-image and a stabilized image. The two after-images now appear "locked together," in striking contrast to the previous instability of the one in relation to the other.

Using the Yabus-type suck-on lenses, the movements tend to be much less than with the full-fitting type. When they occur they are often caused by the rim of the lens touching the eye-lid. During a big voluntary saccadic movement the borders of the target often flash into view, as they do if the target-bearing girder is flicked or touches the nose. However, after the movement is over, the stabilized image bears the same relation to the after-image as previously, suggesting that temporary lag due to the inertia of the lens occurs, but the lens does not slip to a new position. This effect is less evident if the lens is of short focal length and the girder and target are lightly constructed.

The contact lens sometimes slips by rotating about the optic axis. This is reduced by increasing the sucking pressure, by waiting a little time after applying the lens before observing, and by roughening the rim of the lens.

Another troublesome artefact has been a pulsatile movement in time with the heart beat, and more often in a horizontal than in a vertical direction. It is sometimes made worse by forceful retraction of the lids to prevent destabilizations from lid-touching, and seems to be most evident with contact lenses that seat with a rather sharp rim a little way out from the limbus. Measurements made when it was clearly visible suggested that the pulsation had an amplitude of 1 or 2 minutes. It is by no means always detectable. It may be due to pulsation of blood vessels under the rim of the contact lens, or to pulsation of intraocular pressure in association with distortion to the spherical shape of the eyeball caused by lid-retraction, or by an asymmetrical

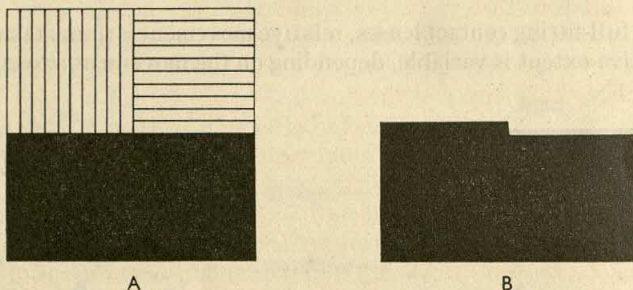


FIGURE 3

Quantitative method for estimating slippage. The target (a) is a straight-edge with two pieces of polaroid mounted as shown. A bright flash of polarized light is delivered first through one half, then after an interval of 5 to 10 sec. through the other half, by rotating the plane of polarization of the flash 90° in the interval. If no slip has occurred the after-image will be straight; if slip has occurred the after-image will have a step as in (b), which can be matched by an adjustable step.

contact lens. Calculation shows that a radial movement of one edge of the contact lens by 0.01 mm. would suffice to account for it.

Though the stabilization with this type of lens is obviously superior to that achieved with the full-fitting type, it should be emphasized that it is quite difficult to prevent any accidental destabilization for a period as long as a minute: An attempt to blink or an unusually large saccadic movement is very liable to intervene, though this is not as serious as in the full-fitting type since the image settles down in the original position afterwards.

Quantitative measurements. It is difficult to estimate visually the extent of the movements described above, and it was therefore thought desirable to confirm the superiority of the Yabus-type lenses by measurement. The principle here is to expose one half of a straight edge mounted on the contact lens, then after an interval to expose the other half. If no movement has occurred the after-images of the two halves will be collinear, but if movement has occurred the after-image will show a step, as in Figure 3 (b). After the two exposures have been given, the subject removes the contact lens and inspects the after-image as it appears on a white surface at about arm's length. He then adjusts a variable step seen at the same distance until it matches the after-image as closely as possible. The target (Fig. 3 (a)) was a straight-edged piece of black film with two thin pieces of polaroid mounted on it as

shown. The subject put on the contact lens, waited for it to seat itself while he adjusted the polaroid in the arc lamp beam (seen through a heavily attenuating filter) until one half-line was of minimum intensity and the other maximum. With the polaroid at this setting a 1/10 sec. flash was delivered at full intensity. The polaroid was then rotated through 90° , and a second flash delivered, the interval between flashes being 5-10 sec., during which time the subject remained looking in the same direction, but without any fixation mark to aid him. The contact lens was then removed, the after-image inspected, and the step adjusted to match it.

The procedure is rather slow and tedious when used for the full-fitting type of lens, because of the difficulty of repeatedly fitting and removing the lens. The best set of determinations gave displacement (in mm.'s at 70 cm., to the nearest 0.1 mm.) as follows: +1.9, -0.9, -0.4, -0.4, +0.3, 0.0. The average size of step was thus 0.65 mm., equivalent to 3.2 minutes of arc. The estimated r.m.s. deviation of the contact lens relative to the eye is ± 3.4 minutes. Movements up to 30 minutes in amplitude have been observed in other runs, and it should be remembered that the subject kept his head and eye fairly steady during the 5-10 sec. that elapsed between the exposure of the two halves of the line.

The Yabus-type of suck-on lens gave better results, though even here a step was often clearly visible, indicating that movement had occurred. The following run of nine trials was amongst the best obtained: +0.1, -0.3, -0.3, +0.2, +0.1, +0.2, +0.1, 0.0, -0.1. The average step (0.156 mm.) was equivalent to 0.76 minutes, and the estimated r.m.s. deviation of the contact lens from the eye was 0.67 minutes. In this run the greatest movement was 1.5 minutes, and it was unusual to get one bigger than 2 minutes.

Controls. The matching of the adjustable vernier to the after-image was not very easy, though it was judged to be reliable subjectively. The following controls suggest that neither random nor consistent setting errors effect the results substantially.

- (1) A piece of aluminium with a step in it was substituted for the straight edge, and was exposed with the polaroid at 45° to the position of maximum extinction of the two halves. From the measured linear dimensions it was estimated that the step subtended 12 minutes. Ten attempts to match the after-image gave a mean result of 10.5 ± 0.31 (10) minutes.
- (2) With the original straight edge the polaroid was set at 45° , and a number of attempts made to match the after-image. In some cases this did not look exactly straight, probably because the exposure of the two halves was not identical, and the average setting error was equivalent to 0.29 minutes of arc. This is distinctly less than the best performance achieved with separate exposure of the two half lines. Assuming that the setting error is independent of degree of image movement, one can correct the former by subtracting the variance due to the latter. This reduces the estimate of r.m.s. deviation hardly significantly, from 0.67 to 0.57 minutes.

Conclusion. These experiments suggest that the full-fitting type of contact lens does not stabilize the image adequately. It may be objected that we have used unfortunate specimens of these lenses, or that we have not waited long enough for them to "bed-down." But there is a prior expectation that they will slip, from the fact that they seat on the conjunctiva not the eye itself, and no good evidence from the publications of those using this type of lens which would show that slip does not occur.

The quantitative results suggest that stabilization is of the order $\pm 3\frac{1}{2}$ minutes for the full-fitting type, and about one-fifth of this for the Yarbus-type. There seemed little point in improving the accuracy of these estimates by repeating them many times, for it probably varies with every lens, and any enterprising user of contact lenses will believe that he can better our results. He may do so, but any such claim requires positive evidence to substantiate it.

Other Artefacts

The following factors can all cause misleading results when working with contact lenses. They are probably known to others who have used contact lenses themselves, but the published accounts give them no prominence. Hence it seems worthwhile listing them and briefly describing them.

Other causes of movement in the image. The slip was measured under conditions favourable to stabilization. As described on p. 39, movement was often caused accidentally by the lid touching the lens, etc.

The lens is free to move slightly inside the eye, especially when it is accommodated for near vision and the suspensory ligament is slack: pupil changes may cause a variable amount of vignetting in conjunction with the artificial pupil: changes in accommodation may vary the sharpness of the image. These three artefacts may be eliminated by using homatropine.

Retinal ischaemia. When a contact lens is sucked into position the eyeball is distorted and the intra-ocular pressure is raised; retinal ischaemia and fading of vision can occur. A pressure greater than — 150 mm. Hg. is needed to cause complete and rapid fading, and such a pressure causes a dull headache-like pain even when the cornea is fully anaesthetized. With a lower pressure there may be some dimming of vision accompanied by a pulsation in brightness for a few seconds after it is applied, but thereafter vision tends to recover unless the suction is increased. These effects are easily distinguishable if non-stabilized images are inspected through suck-on contact lenses, and it is doubtful if this is an important disturbing factor.

Poor optics. Some of the causes are obvious, but none-the-less troublesome. (i) The outer surface of the lens is easily wetted or smeared. (ii) In the suck-on type the inner surface steams up very rapidly unless the lens is first warmed, and this only delays it for about $\frac{1}{2}$ min. (iii) Suction changes the curvature of the front surface of the cornea, thus altering the focus. Filling the space between cornea and contact lens with bicarbonate prevents (ii) and (iii). (iv) Corneal misting usually occurs after 5 or 10 min. wear of the suck-on type; when advanced, it is subjectively and objectively obvious on removing the lens, but minor degrees which are not obvious in normal vision may still affect the subjective appearance of stabilized images (see transparency effect described below, p. 44).

Chromatic aberration. This is uncorrected in the normal eye, and with broad receptor sensitivity curves it inevitably mars resolution. If a short focus uncorrected lens is added to the system, its own chromatic aberration will be added to that of the eye, and is liable to become the limiting factor. The additional error is greater the shorter the focal length, which is why lenses focussing at *ca.* 20 mm. have been used, rather than 5–10 mm. as favoured by Yarbus. This error can be reduced by using a plano-convex lens made of glass with a dispersion lower than water, so that the negative saline surface tends to correct the aberration at the positive glass surfaces (e.g. Zinc Crown glass, plano-convex, radius of curvature 2.1 mm., convex surface in saline; this focuses at about 20 mm. in front of plane surface).

Diffraction. To overcome chromatic and other aberrations there is a constant temptation to narrow the entrance pupil, but this will mar resolution if it is carried

too far, because of diffraction. Yarbus has sometimes used a pinhole, for which the resolving power is inevitably very poor.

Trans-scleral light. When a small pupil diameter is used the light leaking through the sclera becomes relatively more important. It will decrease the contrast of the image, and thus may mimic fading. Furthermore, variations in the trans-scleral light become relatively more important in the subjective effects they produce when the light in the image is held constant, and under these conditions regeneration of the image may be simulated by a decrease in the trans-scleral light. It can be shown that changes in trans-scleral light sometimes cause a reddish or complementary greenish colouration of parts of the image, and on occasions it causes the appearance of shadows of the retinal vessels.

Control of image quality. Because it is difficult to achieve good optical performance it is very desirable to check that vision through the contact lens is nearly normal when the image is not stabilized but moving in the normal way. This can be done by holding an object in the plane of focus, but it is more satisfactory to stabilize the head with a biting board and forehead rest, and cast a good quality image in this plane; one can then inspect the image of distant objects and compare directly with the normal eye. On our best lens it was possible to resolve a wire of breadth 1.7 sec. The same eye without a contact lens resolved a wire of 1.0 sec. under similar conditions. The reduction is partially accounted for by diffraction at the contact lens aperture (1.7 mm.), which is below the optimum for the normal eye (2.3 mm.).

Subjective Appearance of Stable Images

The following account of the subjective appearance of stable images is based only upon my own observations, and it is to quite a large extent simply a confirmation of phenomena that have already been described (see Riggs *et al.* 1953; Ditchburn and Fender, 1955; Ditchburn and Pritchard, 1956; Pritchard, Heron and Hebb, 1960; Yarbus, 1957a and b). But it is hard to get a coherent impression from these accounts, and one is constantly uneasy about destabilization in Riggs's, Ditchburn's, and Pritchard's experiments, and about image quality in those of Yarbus. Hence this description seemed worth giving. It is a summary of my impressions after rejecting the large proportion of observations suspected of being partly interfered with by the artefacts just described.

Initial and final appearance. When one inspects an image of good contrast and optical quality, moderate retinal illumination (say 1 to 100 trolands) and as well stabilized as we are able to achieve, it is seen with full clarity only for the first few seconds, at the end of which time it lacks some of the fine detail and contrast of the original. There is then a period of a minute or so during which the appearance fluctuates, disappearing and regenerating in a way that will be described later. Finally these fluctuations die out, leaving a stable appearance of a fog or grey sky with ill-defined dark and light clouds in it corresponding to the black and white parts of the original image. This final state, a very blurred, very low contrast version of the original image, seems to persist without fluctuation for as long as the conditions are held unchanged. On the occasions when it has disappeared, the cornea has been found to be misted, or the lens surface smeared, on removing the contact lens.

If the light is turned out after the stable state is reached, the negative of the original is seen. This lacks some of the fine detail, but this is usually true of ordinary negative after-images, even those produced by brief flashes of light. If the light is kept off the appearance may fluctuate. If the light is turned on again the original image is seen again, but it is noticeable that this, too, lacks some of the fine detail of

signals that "adapt out" and thus cease upon stabilization are concerned with local spatial contrast rather than local luminance.

The target is a square of black paper with sides subtending about 10° . Near its centre is a small pinhole subtending about 1° , whose image falls, not on the fovea, but about 3° parafoveal. The target is viewed against a uniform white surface, and is therefore seen initially as a large black square against a white background with a white spot at its centre. The appearance of the black square follows the usual sequence, blurring, fading, regenerating, and ending up as a dark grey cloud against a lighter grey background. The white dot fades more rapidly, and may become indistinguishable from the black square before the first complete disappearance of the black square, or it may not be seen when the square regenerates. At this stage the dot has the same apparent brightness as the square, though its actual luminance

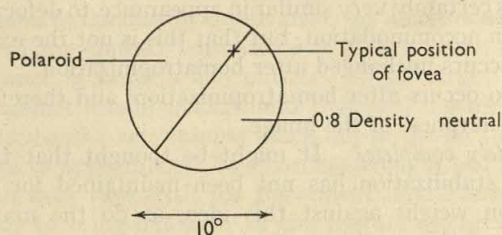


FIGURE 4

Target for doing brightness matches on stabilized images. The fovea is usually eccentric in the disc, and by rotating the disc one can cause the fovea to lie on the dividing line. The intensity of the polaroid half-field is varied by rotating the plane of polarization of the transilluminating light. For unstabilized viewing the target is detached from the contact lens and held in the plane of focus.

is that of the ground. Clearly these observations could not be accounted for if apparent brightness depended solely on local luminance, i , and change of luminance, di/dt , for of two regions of the same, unvarying, luminance one (the dot) has an apparent brightness identical with that of the square, whereas the other (the background) does not. On the other hand rapid adaptation in the fibres that signal local contrast would account for them. This point can be put in over-simple and over-exact language by emending Mach's (1886) suggestion that $-(\partial^2 i / \partial x^2 + \partial^2 i / \partial y^2)$ contributes to the sensation of brightness. This should be modified to $-\partial(\partial^2 i / \partial x^2 + \partial^2 i / \partial y^2) / \partial t$, indicating that the same mechanism is responsible for spatial and temporal contrast, and that it is the product of the two (rather than, for instance, the sum) that is important.

Brightness matches on a stabilized image. A target was arranged as shown in Figure 4, and was trans-illuminated with polarized light. By rotating the plane of polarization of this light it was possible to adjust the brightness of the polaroid half-field until it matched that of the neutral. The matching was done in stabilized and unstabilized conditions. Expressing results as standard deviation of 10 settings $\times 100/\text{mean}$, an unstabilized control gave ± 7.3 per cent.; a stabilized run then gave ± 17.3 per cent.; finally an unstabilized control gave ± 4.4 per cent. There was thus a roughly threefold increase in relative error on stabilization, or a $\times 10$ increase in variance. A variance ratio test showed this to be highly significant. These results confirm those of Clowes (1961).

During these determinations the polaroid was moved to and fro near the matching point, and first one half became brighter, then the other half. If one half was made

brighter and left brighter, the apparent difference in brightness grew smaller. If, after a minute or so, matches were attempted, then the polaroid was adjusted towards equality but initially matches were accepted with the previously bright half-field set at a luminance much higher than the other. However, as time passed, the matches were made closer and closer to genuine equality. Thus it appears that the signals used to make the match are only temporarily disturbed by light and dark adaptation: "brightness" may be temporarily affected by change of luminance, but signals ultimately get through that depend upon real luminance.

Habituation to borders. While making the foregoing matches a curious observation was made. If the two half-fields have almost the same luminance, then small changes of the variable field enable the dividing border to be seen fairly sharply as first one half-field, then the other half-field becomes brighter. But when the match is made, at very different luminances owing to adaptation effects, small changes in the variable half-field do not bring out the border. Hence in the initial matches made after light adapting one half-field one is trying to equalize in brightness two dim, cloudy, blobs of light: the later matches are more like an ordinary photometric match, where the border is seen distinctly.

The phenomenon can be described as habituation to a black-white border, and thus classed with the effects of repeated re-illumination and movement. It is as if the continued presence of a feature of the light image—in this case a border—desensitized the visual pathways to that feature, so that small changes (Δp) in the strength of the feature (p) are not detected: apparently Δp increases with p . This would seem to require a change in the spatial organization of the receptive fields, perhaps like that known to occur in dark adaptation (Barlow, FitzHugh and Kuffler, 1957). The importance of these habituation effects is that they cannot be explained simply on the "diffusion" and "two-channel" hypotheses to be discussed below.

DISCUSSION

These results will first be considered in relation to previous work on stabilized images. Secondly the question will be discussed whether residual movements are important when the image has been stabilized as well as possible. Finally, possible physiological explanations of the results will be examined.

Relation to Previous Work

Completeness of stabilization. The finding that the conjunctival-seating type of lens slips by $\pm 3\frac{1}{2}$ minutes in a 5–10 sec. period, and sometimes moves by as much as 30 minutes, obviously means that stabilization cannot be accepted as being complete in experiments using this type of lens, unless specific controls are performed to show that it is. This does not, of course, mean that all such experiments are valueless, for they show the effect of reducing image movements, even if they do not eliminate them. It should, however, be pointed out that quantitative measures such as the visibility factor, or the percentage of time a test target is seen, will be particularly susceptible to incomplete stabilization owing to the ease with which movement causes reappearance.

Subjective appearance of stabilized images. The account given here differs substantially from previous accounts, but it seems likely that these discrepancies can be accounted for by the various artefacts of the techniques. The phenomena will be discussed in turn to show how this may happen.

(1) *Blurring, or initial loss of detail.* To observe this with clarity the image must initially be of good definition. Because of chromatic aberration with short focus

lenses, diffraction with small pupils, and changes of corneal curvature and misting with air-filled lenses, it is probable that Yarbus has not achieved the required image quality. On the other hand slip of the full-fitting type causes sharp borders to reappear, and this may account for Ditchburn and Riggs failure to emphasize it. One should, however, recall the account of complete and permanent disappearance of fine lines given by Riggs *et al.* (1953).

(2) *Regeneration.* Yarbus did not observe regeneration of the image, and this may possibly be due to the low luminance of the image and the great influence of trans-scleral light in decreasing its contrast when using pin-hole apertures. The regenerated image lacks contrast compared to the original, and if contrast is low enough initially a regenerated image will not be visible.

Regeneration can be triggered by image displacement, and then it has sharp borders and may be almost as vivid as the original. This may explain why Ditchburn and his group emphasize the similarity of the regenerated image to the original, rather than its milky, shadowy, textureless quality.

(3) *Final appearance of dim clouds.* Loss of image contrast from trans-scleral light, or from corneal misting or contact lens misting or smearing, obscures this. Incomplete stabilization also obscures it, because the appearance of the borders on displacement is so much more vivid than that of the dim clouds.

(4) *Comparison with entoptic stable images.* The entoptic phenomena described in the introduction lead to the expectation of complete and rapid fading, but the complete and rapid changes seen in the stabilized images are blurring and loss of contrast. The shadows of the capillaries are very small, those of the retinal vessels rather small, and Maxwell's spot has low contrast, compared with the stable images that have been examined. It is just possible that rapid initial blurring and loss of contrast would reduce them permanently below threshold, but one cannot feel wholly satisfied with this explanation and other factors such as retinal locus may be important.

Are Residual Movements Important?

These occur even with the best type of lens so far devised: are they affecting our conclusions about regeneration? There are three reasons for believing they are not.

- (1) If the image has been well stabilized for some time, then spontaneous regeneration and the effect of image displacement are easily distinguishable subjectively; displacements affect borders, which they bring out sharp and clear, whereas regeneration brings out the main masses in a rather shadowy and blurred form. In the early stages of fading, and particularly where there is considerable residual movement, the distinction is very much less evident, probably because accidental destabilization triggers off the process that normally occurs spontaneously, and thus causes confusion.
- (2) With the qualitative method of detecting destabilization, using an after-image and a pointer on the contact lens, one can find conditions where the movements are minimal—e.g. gazing in a relaxed fashion at the floor. Regeneration occurs even when no detectable displacement has occurred.
- (3) After-images persist and can be observed in total darkness for a period of several minutes, though they are much less distinct under these conditions than when they are rendered visible by changing the retinal illumination. The sequence of events described for stable images can be observed on them—initial blurring and loss of contrast: a period of fluctuations when the after-image disappears and regenerates: and a final dim, cloudy, appearance.

These three reasons are not perhaps absolutely compelling, but they give some confidence that the description of the subjective appearance of stabilized images is substantially correct and will not have to be changed if perfect stabilization is achieved. At the moment, however, this is the subjective account of only one individual.

Physiological Mechanisms

The fact that fine detail is not seen after stabilization can be said to imply that the mechanism responsible for signalling high spatial frequencies is not able to signal low temporal frequencies; both spatial and temporal changes are required to activate it. Likewise the fact that, under certain conditions, the luminance of large masses is seen incorrectly initially, but correctly ultimately (see p. 45 and 46) means that the mechanism signalling the average luminance of large areas does not respond to rapid change. Spatio-temporal differentials, and spatio-temporal averages, are signalled, but not the actual local luminance, nor spatial averages of temporal differentials, nor temporal averages of spatial differentials. However, to talk in these terms is not necessarily very helpful to physiologists or psychologists, nor does this description account for all the observations. It seems more profitable to make specific suggestions about the mechanisms involved, since these ideas can be tested by further experiment. Two hypotheses will be proposed; they are not alternatives for the "rapid-detail" channel might be the retina's answer to the problem of diffusion.

Diffusion hypothesis. If an excitatory product of the absorption of light can diffuse laterally through the retina, this would account for the blurring and loss of detail that was one of the prominent findings on stabilized images. The initial rates of increase of local concentration of the excitatory substance would accurately reflect the details of the image, but the equilibrium concentrations would only represent a blurred version of it, for the substance would diffuse out of the illuminated areas into the shadows. Edridge-Green (1920), Ives (1922), and Brindley (1962) have postulated diffusion processes in the retina. The rate required here, though faster than Brindley found, does not seem impossibly fast for a diffusion process.

Two-channel hypothesis. The phenomena described can be partly accounted for if there is a dual system of fibres connecting eye and brain. (1) A rapidly adapting channel signalling increases and decreases in illumination in small regions relative to the average over larger regions. These units would signal an approximation to the spatio-temporal derivative of the light, $-\partial(\partial^2 i / \partial x^2 + \partial^2 i / \partial y^2) / \partial t$, and would have properties similar to the on-centre and off-centre units of the cat's retina (Kuffler, 1953; Kuffler *et al.*, 1957). Their rapid adaptation would account for the rapid loss of detail vision and decline of contrast after stabilization. (2) A slowly adapting or non-adapting channel picking up from large receptive fields. De Valois (1960) has evidence of slow-adapting fibres in the monkey, though it is not known if they have large receptive fields, nor if they transmit information to the cortex. These would be the mechanism subserving the "dim-clouds" which appear after prolonged stabilization, and are correctly related to real luminance of the parts of the visual field.

Fading and regeneration. It is known that alternations in appearance rather similar to those seen in a stable image occur in situations where conflicting or ambiguous information is presented to the senses. Well-known examples of this are binocular rivalry, and the Necker cube. Could the fluctuations in the present case be the result of ambiguity and conflict in the messages being received centrally? A moment's consideration will show that the two channels are sending up information which, if not necessarily conflicting, is at any rate different from anything that would be received in the presence of ordinary eye and image movements. The slow, coarse

channel signals the presence of, say, a large black object, but once the rapid-detail channel has adapted, this large black object is left without any borders—even when the eye executes a movement that would, if it had the sharp borders that were originally seen, bring a further volley of impulses in the rapid-detail channel. Furthermore, there has been no volley of impulses such as would have occurred if the large black object had actually disappeared. Thus on the one hand there is evidence for the continued existence of the object in the continuing signals in the slow-coarse channel, and in the lack of any signals in the rapid-detail channel which would have indicated that it had gone; and on the other hand there is evidence *against* its continued existence in the lack of continued volleys in the rapid-detail channel, even when the eye moves.

The fluctuations in appearance may thus correspond to the centres "trying out" the various visual fields that would be compatible with zero activity in the "fast-detail" channel. This implies $-\partial(\partial^2 i/\partial x^2 + \partial^2 i/\partial y^2)\partial t$ is zero and therefore either the bracketed term, or $\partial/\partial t$, or both, is zero. The centres first "try out" the possibility that $\partial/\partial t$ is zero: there has been no change in the visual field, and one "sees" what was originally present persisting. Small eye movements falsify this because they would have caused image movement and activity had the visual field been unchanged. The centres then "try out" the possibility that the bracketed term $(\partial^2 i/\partial x^2 + \partial^2 i/\partial y^2)$ is zero: there are no borders in the visual field and one sees a uniform field. The two possibilities are seen alternately a few times, but the slow coarse system, and the ineffectiveness of eye movements, eventually allow only one interpretation: nearly uniform gradients $(\partial^2 i/\partial x^2 + \partial^2 i/\partial y^2)$ still nearly zero between regions of different luminosity. Thus the final dim cloudy appearance is arrived at by compromise and successive approximation.

The mechanisms that turn volleys of impulses into our visual impressions of the world around us are necessarily complex, and it may be more than usually misleading to interpret these purely subjective phenomena in terms of the physiological mechanism that would account for them. But at this stage it is worth suggesting such mechanisms in the hope that experiments will be devised to amend them. Diffusion and the two-channel hypothesis might account for the "blurring" of stabilized images, the fluctuating appearances, and the final dim, cloudy appearance: habituation to re-illumination, to movement and to borders, are unexplained.

SUMMARY

(1) Methods are described for detecting and measuring the displacements of supposedly stable retinal images.

(2) The full-fitting type of contact lens that seats on the conjunctiva allows about $\pm 3\frac{1}{2}$ minutes displacement of the image even with the gaze directed ahead; the completeness of stabilization must therefore be doubted in experiments using this type of lens.

(3) The Yabus-type of light-weight, limbal-seating, suck-on, contact lens produces considerably better stabilization than the full-fitting type, but it is difficult to achieve stabilization better than ± 40 sec., especially for long periods.

(4) Other artefacts can also affect the appearance of "stabilized" images, of which the most important are poor optical quality due to chromatic aberration and diffraction, and loss of image contrast from trans-scleral light.

(5) Good-quality images stabilized as well as possible have been observed subjectively. They "blur" and lose contrast rapidly: detail and texture cease to be visible, and do not reappear.

(6) The shadowy, textureless, image that remains disappears and regenerates for a period of about a minute.

(7) A dim cloudy image persists after the fluctuations cease. Brightness matches can be done on this image with an accuracy about one-third that for the same image unstabilized.

(8) Simple qualitative experiments suggest that the same mechanism signals both spatial and temporal contrasts. When the image is stabilized these signals vanish, because there are no changes in luminance. By imposing change, paradoxical appearances can occur: e.g. a dark part of the visual field can be made to appear lighter than a grey part.

(9) "Diffusion" and "two-channel" hypotheses can account for most of these phenomena, but habituation occurs to repeated re-illumination, to movement, and to borders: this is not explicable on these simple hypotheses.

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SHORTER ARTICLES AND NOTES

HABIT REVERSAL IN THE TURTLE

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Two groups of mature "painted" turtles were trained in a T-maze. One group had five trials per day, with correct and incorrect turns reversed for each animal whenever it reached the criterion of five errorless trials on a given day. The second group had 10 trials per day, with correct and incorrect turns reversed daily, irrespective of the performance of the animals. Only in the data for the second group did there appear some indication of progressive improvement in habit reversal. The results are considered in relation to those obtained in analogous experiments with other species.

INTRODUCTION

Progressive improvement in habit reversal, a well-known mammalian phenomenon, recently has been demonstrated in the bird (Warren, Brookshire, Ball, and Reynolds, 1960; Bullock and Bitterman, 1962). Several experiments with the fish (Wodinsky and Bitterman, 1957; Bitterman, Wodinsky, and Candland, 1958; Warren, 1960) have failed, however, to show progressive improvement, except for one instance (see the first of the three papers cited) in which there appeared a pattern of improvement qualitatively unlike that found in the rat. The experiments to be reported here are concerned with the course of habit reversal in an intermediate species, the turtle *Chrysemys picta picta*. Earlier work with another turtle by Seidman (1949) might have provided some relevant information but did not in fact do so, because the maze employed made it possible for the subject to see from the choice-point the block which served to convert the incorrect arm into a cul-

METHOD

The subjects were selected on the basis of tameness from a larger group collected in southern New Jersey in the late summer of 1960. They ranged in size (width of carapace) from 4-6 in. The animals spent the fall and winter months roaming freely in a small, temperature-controlled greenhouse equipped with several shallow pools, and in that environment they seemed to thrive. With the coming of spring (1961), their appetite for the ground beef on which they were maintained became so good that the possibility of an experiment with food as reward suggested itself. Thereupon about a dozen of the animals were given their daily feedings in small individual tanks to determine whether they would take food under such conditions and, if so, how much they would take. Tested in this way during the previous fall, the turtles had failed to eat consistently, which made it necessary to turn to other motivational techniques, such as water-deprivation and opportunity-for-immersion-in-water as reward (Gonzalez and Bitterman, 1962). Now, however, the animals ate consistently and took enough food that an experiment with repeated daily trials seemed to be feasible. It is interesting to note that, as this paper is being written (in November, 1961), the animals continue to take food readily, which means that the increase in appetite manifested last spring cannot be attributed to seasonal changes alone, but must have had something to do with length of stay in the laboratory.

The apparatus constructed for what was thought would be simply a few preliminary trials is shown in Figure 1. It was a maze of one choice-point, with alleys 8 in. wide and walls 10 in. high. The stem was painted grey (both floor and walls), the right arm black, and the left arm white. Beyond the 90°-turn in each arm, the floor of the maze sloped downward to the end of the arm, and the depression was filled with water to a depth of about 1 in. Just above water-level on the end wall of each arm was a narrow ledge which held a pellet of ground beef when choice of the arm was to be rewarded; the pellet was clearly visible from the turn in the arm, and, upon reaching that point, a properly pre-trained and motivated animal would approach it, seize it, pull it down into the water, and eat it. The second segment of each arm was fitted also with a false floor, perforated with many holes and painted the same colour as the arm, which converted the sloping, water-filled chamber into a relatively dry one with a level floor. The insert was used in a given

arm when choice of that arm was incorrect, its purpose being to differentiate the consequences of an incorrect turn from those of an incorrect in respects other than the mere absence of reward.

An error was defined as entry into the incorrect arm far enough that the carapace of the animal was carried entirely beyond the line in the floor of the maze which separated the grey, V-shaped extension of the stem from the black or white arm. The situation was such that none of the consequences which differentiated an incorrect from a correct choice could possibly arise unless the animal entered the incorrect arm far enough to be scored as having made an error. No photocells or other objective indicators of error were installed in the maze, which was intended only for preliminary observations. It was anticipated that these observations would suggest the need for substantial modifications in design, and that a new maze then would be constructed which could be instrumented appropriately. As it happened, however, the first version of the maze proved reasonably satisfactory, and work with it was continued. Fortunately, the visual estimation of error was easy to make and could be made with high confidence; the animals hesitated only rarely at the criterion line, either moving well into the incorrect arm or not entering at all.

The ten animals selected for the first experiment were kept in a restricted area of the greenhouse equipped with a separate pool. Individuals were identified by means of numbers painted on their shells. Preliminary training consisted in feeding pellets of

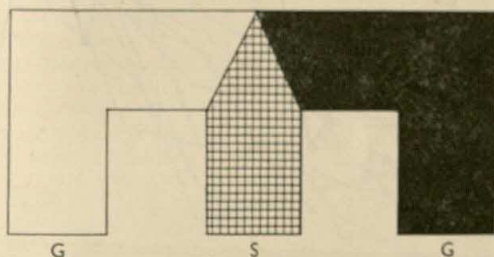


FIGURE 1

Diagram of the maze. (S, grey stem; G, G, white and black goal-compartments at ends of white and black arms; inside-width of alleys, 8 in.)

ground beef in small individual aquaria containing an inch or so of water. First the pellets were dropped directly into the water; after a time they were stuck to one wall of the tank above the level of the water, requiring the animal to pull them down into the water. In the experiment proper, each animal was given five trials per day by the correction method; that is, the animal was permitted to retrace its steps after entering the incorrect arm, and each trial ended with reward at the end of the correct arm. The animals were run in rotation and spent the intertrial intervals in small individual tanks. The length of the intertrial interval for any animal at any point in the experiment depended, therefore, on the time taken by the other animals to complete a trial; it averaged about 15 min. and did not change substantially in the course of the experiment. In the original learning (Reversal 0), all animals were trained against the preference displayed on the very first trial (whichever side was chosen first on that trial was incorrect). As each animal achieved the criterion of five errorless trials on a given day, the previously incorrect arm became correct for that animal (Reversal 1), and it was trained to the same criterion, whereupon the arm which was correct originally became correct once more (Reversal 2), and so forth.

When the first experiment was well under way, a second was begun. Five additional animals were selected from the available stock and pretrained in the same way as the first group. The experimental training was the same as for the first group except that 10 trials per day were given and that the animals were reversed daily. On the first day of training, the black arm was correct, on the second day the white arm, on the third day the black arm, and so forth. Again the animals were run in rotation, and, since the group was smaller, the intertrial interval averaged only about 10 min. Before work with the animals of the

first experiment was discontinued, they were given ten days of training under the conditions of the second experiment, with white correct on odd days and black on even days.

RESULTS

During the four-month period of experimental work, eight of the 10 animals selected for the first experiment completed at least 14 reversals, most of them more than 20. The data for these animals are plotted in Figure 2 in terms of mean initial errors to criterion. (The data for the remaining two animals, which progressed much more slowly, are omitted.)

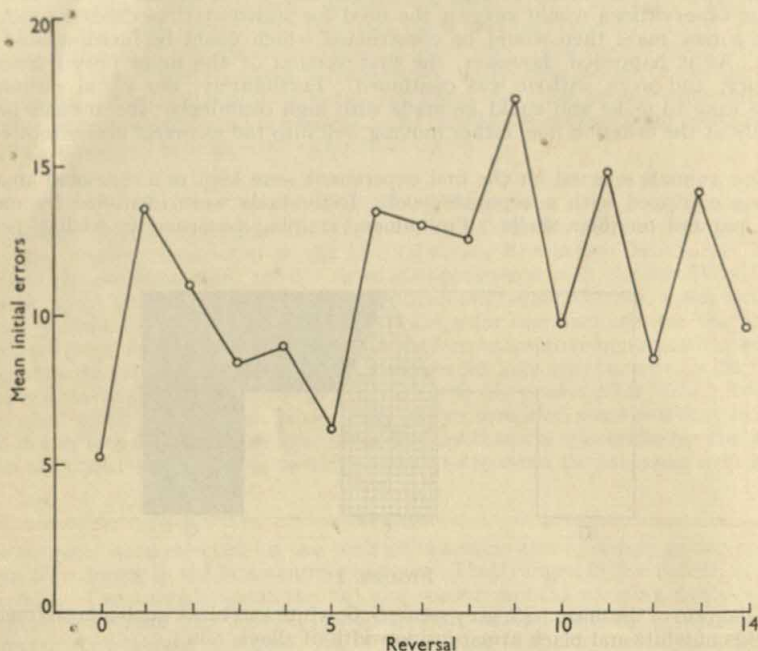


FIGURE 2

Mean initial errors to criterion in the first experiment.

Beyond Reversal 0, the curve is rather level, showing no general trend either upward or downward, although the regular alternation of the points for the later reversals reflects the development of a preference for black-right (which was quite the reverse of the preference shown originally by most of the animals). The performance of the animals which progressed beyond the fourteenth reversal gave no indication of a downward trend in errors per reversal for reversals after the fourteenth.

In Figure 3, learning curves for the first fourteen reversals are plotted in terms of mean initial errors per day. Based on the data of the same animals whose performance is shown in Figure 2, the curves illustrate the general course of learning in each reversal. Since the order of the curves is essentially random, no attempt is made to differentiate any but that for the first reversal (broken line).

With the use of the correction method in this experiment, repetitive errors were possible (as, for example, when an animal entered the incorrect arm, retraced into the stem, and then entered the incorrect arm once more). Such errors never were very frequent, averaging about 2.5 for the first reversal and declining by the third reversal to about 0.5, a level which was maintained thereafter.

The five animals of the second experiment were trained for 70 days, and their performance is summarized in Figures 4 and 5. Figure 4 shows mean initial errors for successive ten-day periods, while Figure 5 shows mean within-days learning curves for each of the seven ten-day periods plotted in terms of initial errors per trial. A hint of progressive

improvement is evident in these functions. The curve of Figure 4 seems to decline systematically in negatively accelerated fashion. In Figure 3, the curve for the first block of 10 days (broken line) is generally higher than the others, and the curve which approaches it most closely is that for the second block of 10 days. The number of animals is small, but

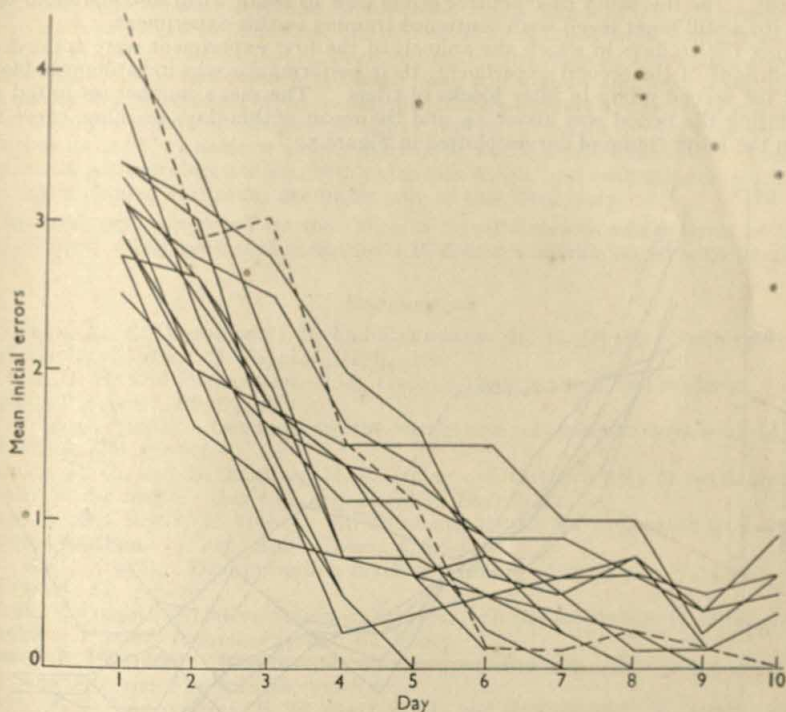


FIGURE 3

Mean initial errors per day in reversals 1-14 of the first experiment. (The curve for the first reversal is shown by a broken line.)

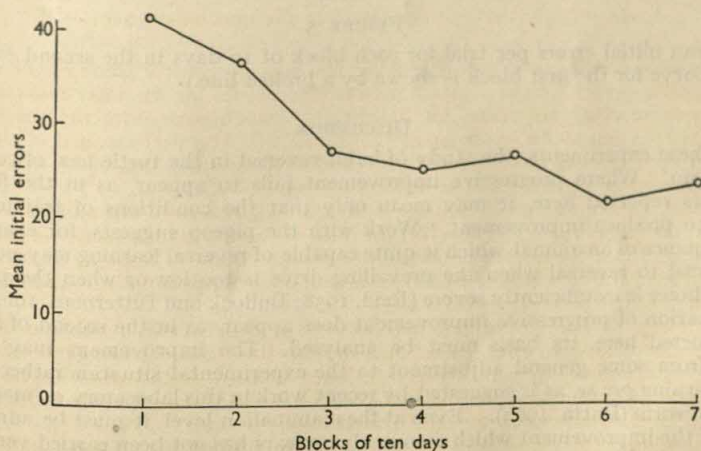


FIGURE 4

Mean initial errors per block of 10 days in the second experiment.

improvement is reflected clearly in the individual data of four, and (although the test is not quite appropriate because it involves some selection of blocks), a nonparametric analysis of variance for the data of the first four blocks (representing the approach to and the attainment of asymptote) yields significance at a level of confidence well beyond 1 per cent. The frequency of repetitive errors (low to begin with) also shows an orderly decline (to a still lower level) with continued training in this experiment.

During the 10 days in which the animals of the first experiment were trained under the conditions of the second experiment, their performance was indistinguishable from that of the second group in later blocks of trials. The mean number of initial errors made during the period was about 24, and the mean within-days learning curve is like those in the lower clump of curves plotted in Figure 5.

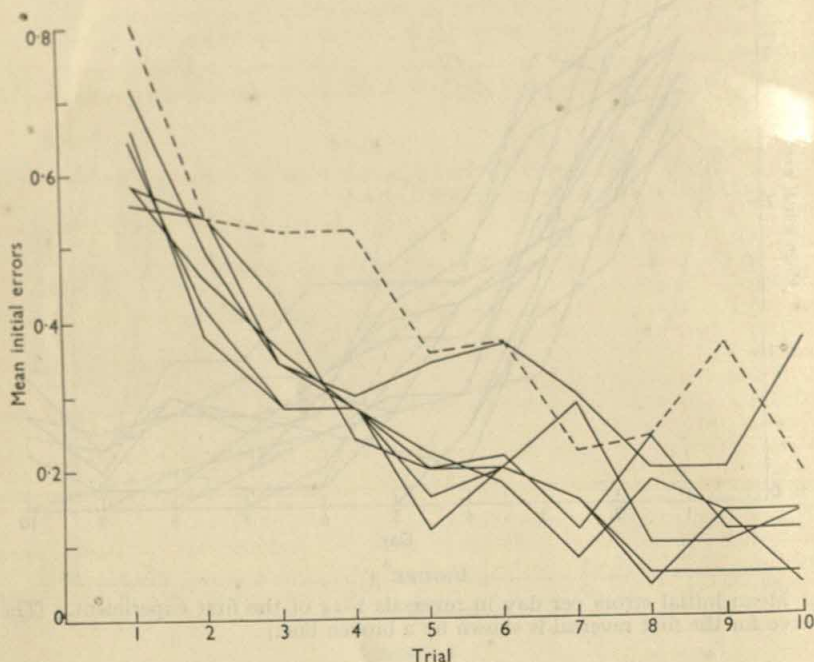


FIGURE 5

Mean initial errors per trial for each block of 10 days in the second experiment. (The curve for the first block is shown by a broken line.)

DISCUSSION

With these experiments, the study of habit reversal in the turtle has, of course, only barely begun. Where progressive improvement fails to appear, as in the first of the experiments reported here, it may mean only that the conditions of training are not adequate to produce improvement. Work with the pigeon suggests, for example, that the performance of an animal which is quite capable of reversal learning may not improve from reversal to reversal when the prevailing drive is too low or when the penalty for incorrect choice is insufficiently severe (Reid, 1958; Bullock and Bitterman, 1962). Where some indication of progressive improvement does appear, as in the second of the experiments reported here, its basis must be analysed. The improvement may result, for example, from some general adjustment to the experimental situation rather than from reversal learning *per se*, as is suggested by recent work in this laboratory on maze-learning in the earthworm (Datta, 1962). Even at the mammalian level, it must be admitted, the analysis of the improvement which commonly appears has not been carried very far.

Whether the improvement which appeared in the second of these experiments with the turtle is reliable, and, if so, whether it reflects anything beyond a general adjustment to the maze, are questions which remain to be answered. Before doing further work with

this technique, however, one might want to look for a better one. The maze certainly is an inefficient instrument, and, more important, there is some possibility that the rewarding effects of food are offset in the maze by a residual resistance to handling which may be appreciable even in the tamest of turtles. Subsequent investigation has shown, fortunately, that a key-nosing response (involving a manipulandum like the pigeon-key) is easy to elicit in the turtle, and that the animal will take food from a mechanical feeder like that of Hogan and Rozin (1961); these findings set the stage for automated experiments directly analogous to those which are being done with pigeon and fish. Even under the most favourable circumstances, of course, the turtle's interest in food may not approach that of a hungry pigeon or fish, and perhaps it will be necessary to turn to more powerful incentives in order to achieve proper control of the animal's behaviour. Preliminary experiments with air-deprivation, with cutaneous shock, and with self-stimulation of the brain via implanted electrodes are under way in this laboratory.

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DRIVE LEVEL EFFECTS ON TASKS OF NARROW AND BROAD ATTENTION

BY

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Easterbrook (1959) has predicted that (a) performance on tasks involving the use of a wide range of cues will be impaired under high-drive conditions and that (b) performance on tasks requiring the use of only a narrow range of cues will be facilitated under such conditions.

Using the Porteus Maze Test, relatively clear evidence was obtained to support the first prediction. Using the Stroop Colour Word Interference Test some evidence was obtained in support of the second prediction.

In a recent review of the literature, Easterbrook (1959) has proposed that high drive leads to reduction in the range of cue utilization, or in other words to a narrowing of attention. Whether performance is facilitated or disrupted by high drive depends upon the range of cues demanded by a given task. For example, a reduction in the range of cue use, as a result of high drive, will impair performance on a complex multi-cue task on the assumption that some task-relevant cues will be omitted from attention. Conversely, such a reduction of cue use will lead to improved performance on a simple task, one requiring a few selected cues, on the assumption that task irrelevant cues and distractions are omitted from attention.

Using a multi-cue task, Bahrlick, Fitts and Rankin (1952) reported that increasing rewards for total task proficiency led to gains in central task tracking but at the cost of proficiency in responding to peripheral task lights. Callaway and his co-workers (1958a, 1958b) having subjects respond rapidly to colour tints, while attempting to ignore the inappropriate colour names on which they were superimposed, reported superior performance for high drive (stimulant drug) subjects.

It is the purpose of this study to test Easterbrook's proposal that increases in drive level adversely affect performance on tasks involving the simultaneous use of a large number of relevant cues, such as Porteus Maze Test (1959), but lead to improved performance on tasks requiring the selection of relatively few cues, such as the Stroop Colour Word Interference Test (1935).

METHOD

Subjects. The subjects were 50 male undergraduate students, ranging in age from 17 to 32 years, who were assigned at random to one of three groups which did not differ in terms of age distribution.

Measures. All subjects received the Ishihara Test for colour blindness, in addition to repeat testing on the Stroop and Porteus Tests.

The Stroop Test consists of three cards. On the first card, the names of four colours (red, blue, yellow, green) are printed 100 times in random order while on the second card dots are painted in the same four colours and arranged 100 times in random order. The third card contains 100 examples of the four colours superimposed on the printed names of the colours, the actual colour being superimposed on the name of a different colour. For example, the printed word "red" may be tinted yellow or blue. For Card I, the subject is required to read the names of the colours as quickly as possible. On Card II he is required to report the colour of each of the dots as quickly as possible. For Card III he must name the colours as quickly as possible ignoring the printed word on which the colour is superimposed. Time scores and error scores are recorded in each case. It is assumed that performance on Card III of the Stroop will benefit from narrowed attention by acting to exclude the printed name of the colour. It is also assumed that performance on Card II will similarly benefit by the exclusion of task-irrelevant cues.

Seven of the Porteus Mazes, from year eight to adult, were used from each of the Extension and Revision series. Only one trial for each maze was allowed. Each subject completed the selected mazes from both series, making a total of 14 mazes. He was

instructed, at the beginning of each series, to find a way out of the maze as quickly as possible, not to bump into any lines, not to go into any blind alleys, and not to lift the pencil from the paper. Instructions were repeated before year eleven maze. Time taken for each maze was recorded to 1/5 sec. It is assumed that this is a broad-attention task, which requires the simultaneous use of a variety of spatial and temporal cues.

Drive conditions. High drive was induced by the following method. A sample electric shock of 125 volts was given for a duration of 30 millisecc., through electrodes attached to the index and ring fingers of the subject's left hand. Then it was stated that the shock would be stronger and would be repeated if the subject's level of performance fell below the average for his group. Shock and threat of shock, therefore, constituted the main procedures for inducing drive. Furthermore, it was pointed out to each subject that the tests used were forms of intelligence tests, and that his status as a university student should assure performance above the average. It was also suggested that males were lagging behind females in performance in these experiments.

Subjects were told that mild shocks would occur at the end of certain trials to see if electrodes were still making contact. The mild shocks of 45 volts intensity were used to remind subjects that poor performance would lead to strong shock.

Low drive conditions were produced with the following instructions: it was explained to each subject that a new use for these tests was sought; that the experimenters were seeking further information concerning the modification of scoring, of instructions, and time required to administer the test. A suggestion that this might even become monotonous, was made to the subject. These instructions were given when conditions of low drive preceded conditions of high drive in the experimental design. When the situation was reversed, and the low-drive conditions followed high-drive conditions, a further set of instructions preceded the standard low drive instructions noted above. The subject was complimented on his performance, electrodes were removed, casual conversation was encouraged, and he was told that the next part would be easy. After a brief time spent in rearranging materials, and when it was felt that the subject was relaxed, standard low drive instructions were given.

Experimental design. Each experimental session was divided into two testing periods. During the first period, all subjects completed the Stroop and Porteus Tests, with test order counterbalanced. Following the first testing, and after reversing drive conditions for the experimental groups, all subjects were retested on the Stroop and Porteus measures. Two experimental and one control group were used. Group A performed under high drive conditions during first testing and under low-drive conditions during second testing. Group B performed under low-drive conditions for first testing and under high-drive conditions during second testing. Group C performed under low-drive conditions for both first and second testings. The experimental schedule permitted an analysis of the combined experimental effects early and late in testing, by means of a switch-back or cross-over analysis of Groups A and B, while comparisons between Groups B and C afforded an opportunity to analyse experimental effects late in practice.

Heart rate. In order to obtain an estimate of the duration of the effects of shock- and threat-of-shock procedures 10 additional subjects were tested and continuous heart rate recordings taken before, during, and after testing. Five subjects performed under conditions similar to those of Group A and the remaining five under conditions similar to those of Group B. The use of heart rate was based on work by Schnore (1959) who reported that this measure consistently differentiated between various high- and low-arousal conditions.

RESULTS

Group A and B Comparisons

Group A operated under high-drive conditions on first testing and under low-drive conditions on second testing, whereas the order of the drive conditions was reversed for Group B.

Maze test. The basic response measure for the Maze was that traditionally used, the error score. Errors were defined in the manner proposed by Porteus and included: bumping into lines, lifting the pencil from the paper before completing the maze, crossing over any lines, and going into any blind alleys.

Analysis of variance for switch-back designs (Brandt, 1938) showed a significant impairment in performance, based on total error score, under high-drive conditions ($p < 0.01$, $F = 8.69$, d.f. 1 and 33). In effect, this analysis compared the combined effect

of high-drive conditions on first and second testing with the combined effects of low-drive conditions on first and second testing.

The low- and high-drive conditions were compared on first and on second testing separately. In the case of first testing, using a one-tailed test, the low-drive group had significantly fewer errors than the high-drive group ($p < 0.05$, $t = 1.98$, d.f. 32). The difference did not reach significance under second testing conditions. These results suggest that drive effects are more predominant early than late in practice, using this kind of task. However, this question should be held in abeyance until the Group B and C analyses are reported since these groups received identical treatment on first testing.

Stroop. Following Callaway and Stone (1960) the basic Stroop response measure was the arithmetic sum of the response times for Card II (colours alone) and Card III (colours-on-words). This method of scoring is based on the assumption that high drive will improve performance on both cards, due to reduction in the number of distracting and irrelevant stimuli. The analysis of variance showed, as predicted, that performance under the high-drive conditions was superior to performance under the low-drive conditions ($p < 0.05$, $F = 5.49$, d.f. 1 and 33). One-tailed t tests for differences between high- and low-drive conditions on first testing, and on second testing were not significant although all differences were in the predicted direction. The switch-back analysis of variance yielded significant practice effects from first to second testing ($p < 0.001$, $F = 34.93$, d.f. 1 and 33).

Comparison of Group B and Group C

Since the experimental conditions (low drive) were the same on first testing for these two groups and with Group B receiving high-drive conditions on second testing, the effect of drive late in practice can be estimated without being confounded by differential treatment early in practice, as was the case in comparing Group A with Group B. Using t tests the groups did not differ on any measure on first testing. The scores used to estimate drive effects were the difference scores between first and second testing for each of the Groups B and C. In using difference scores, Lindquist (1953) advises the use of analysis of covariance if the difference scores and the initial test scores are correlated.

The covariance analysis yielded no significant drive effects on second testing with the Stroop. However, using the Porteus error scores, the results showed the predicted impairment in performance of the high-drive group ($p < 0.05$, $F = 4.63$, d.f. 1 and 30). These results support the evidence obtained from the comparisons of Group A and B, where the effects of drive appeared to be stronger on first than on second testing.

Heart Rate

The analysis of heart rate was based on two measures taken under low-drive and two measures under high-drive conditions for each subject. Each measure in turn was based on heart rate readings taken at three standard points, at the beginning, in the middle, and late in the performance on the Stroop and Porteus tasks respectively. No reading was based on less than a 30-sec. heart rate sample. A Friedman's two-way analysis of variance by ranks (Siegel, 1956), was done comparing the combined high-drive groups with the combined low-drive groups, a procedure which tests for the main effect of the drive conditions regardless of order. The difference between the high- and low-drive conditions was significant ($p < 0.001$ level, $X^2 = 17.6$, d.f. 3 and 1). This analysis, as well as an examination of the ranked data, indicates that heart rates were significantly higher under the high-drive conditions, throughout the testing intervals, both early and late in practice.

DISCUSSION

The results of this study generally lend support to Easterbrook's hypothesis that high-drive facilitates performance on tasks requiring the use of a few selected cues, but impairs performance on tasks requiring the simultaneous use of many cues.

Easterbrook specifically cited the Porteus Maze Test as a task involving the use of a wide range of cues. In the present study, significant degrees of impairment were found with high-drive subjects both early and late in practice. However, it was found that the effects of drive early in practice were stronger than those late in practice. This finding lends further support to the hypothesis if it is assumed that practice reduces the range of cues needed for adequate performance.

Easterbrook suggests it is the "peripheral" cues that are omitted by high-drive subjects. What cues are perceived as peripheral in a maze task is determined in part, presumably, by

set or instructions. Therefore, while high- and low-drive subjects in this study differed in error scores they did not differ in time taken to complete the mazes. Such a difference should be expected, however, in another study using instructions placing great stress on accuracy of performance.

Whereas, it was predicted that high-drive conditions would impair performance on the Porteus Maze, it was predicted that such conditions would facilitate performance on the Stroop Colour Test. Once again, the evidence is in the predicted direction, but is not as clear-cut.

Callaway (1959), who found that drugs producing EEG arousal facilitated performance on the Stroop, recently (Callaway and Stone, 1960) reported that work by Broadbent (1958) has been useful in considering problems relevant to the focus or the narrowing of attention. Broadbent has proposed that under arousal not only is there narrowing of attention, but that in addition the stimuli within the narrow range take on an equally probable likelihood of being perceived. Relating Broadbent's hypothesis to performance on Card III of the Stroop, under low-drive conditions (broad attention), interference would be expected both from task irrelevant peripheral stimuli, and from the printed colour names. On the basis of past experience, the latter have a greater probability of being perceived and responded to than have the coloured spots. On the other hand, under high-drive conditions the resulting narrowing of attention should reduce the effects of task irrelevant stimuli, and also counteract the effects of past experience in such a way as to make the perception of the coloured spots relatively more probable than is the case under low-drive conditions. Furthermore, high-drive conditions might be expected to facilitate performance in naming the coloured spots on Card II, since presumably the effects of task irrelevant peripheral stimuli would be reduced due to reduction in range of cues. On the basis of these assumptions, Callaway reanalysed his data, comparing groups on the basis of Card II + Card III scores rather than Card III - Card II scores, as used initially. He found his revised scoring system more sensitive to drug effects, and interpreted this as support for Broadbent's assumptions. In the present study, for the switch-back analysis of Groups A and B, a significant difference using Card II + Card III scores, was found for the combined high-drive groups over the combined low-drive groups. On the other hand, while Card III - Card II scores indicated trends in the predicted direction, they yielded no statistically significant results. These findings support Callaway's recent conclusion that the combined scores of Cards II and III are more sensitive to arousal conditions.

While Malmö's (1959) and Hull's (1943) theories relating to drive were not specifically under investigation, the findings relating to the Porteus Maze are in agreement with both theories. Impairment of the high-drive group is in keeping with Malmö's hypothesis of an inverted U relationship between performance and level of arousal, with high arousal leading to impairment. Hull's theory, as developed by Taylor (1956) and Spence (1956), predicts that high drive leads to impairment of performance on tasks which involve several responses of different strengths rather than a single response. Spence (1956, p. 222) has cited evidence from maze performance to support the theory.

Although the Porteus Maze results are in keeping with theories proposed by Malmö and Hull it is more difficult to account for the superior Stroop performance under high-drive conditions. No clear-cut prediction could be derived from Malmö's theory; however, there appears to be little doubt about the prediction based on Hullian theory. As stated by Spence (1956, p. 222) "if on the other hand, the correct response (*naming the coloured spot*) has a lower habit strength than the competing response or responses (*reading the colour name*) then high drive will be detrimental to performance . . ." (italics added by authors). It seems clear that Hullian theory predicts impaired performance by high-drive subjects on a task involving competing responses such as the Stroop, particularly when the correct response is less practised than the incorrect response.

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A ZEIGARNIK-LIKE EFFECT IN THE RECALL OF ANAGRAM SOLUTIONS

BY

A. D. BADDELEY

From the Medical Research Council Applied Psychology Research Unit, Cambridge

Subjects were asked to solve a series of 12 anagram problems. For each of these they were allowed 1 min. and if they did not solve it in this time they were told the solution. When asked to recall the solution words at the end of the series, subjects remembered items they had failed to complete almost twice as often as those they had solved.

It is suggested that this phenomenon is analogous to the Zeigarnik effect, but that it has the advantage of occurring in conditions which are easy to specify and control.

INTRODUCTION

Since Zeigarnik's demonstration that interrupted tasks are recalled more readily than completed tasks, attempts to reproduce the phenomenon and explore its theoretical implications have been faced with two technical problems: the selection of suitable material, and the method of interrupting the task. Although the importance of the material in the Zeigarnik effect has been stressed (Zeigarnik, 1927; Alper and Black, 1949), the tasks used in studies of this effect have seldom been specified in sufficient detail to be reproduced. The manner of interrupting the tasks is probably an even more important factor, which is certainly more difficult to specify and control, for as Zeigarnik points out, subjects react very differently, some accepting interruption quite placidly, while others have to be physically restrained from completing the task.

The following phenomenon was observed during a series of experiments on anagram solving. It is reported because it appears to function as a Zeigarnik effect while avoiding both of these technical problems since it occurs in a completely specifiable situation in which interruption forms a natural part of the task.

METHOD

Anagrams of 12 common five-letter words, Royal, Metal, Eight, Party, Grain, Story, Twice, Above, Child, Plant, Towel and Score were used. The patterns of letter arrangement were the four found by Hunter (1959) to be equally difficult namely 31524, 35142, 52413, and 42531. The anagrams were made into four separate series, each comprising the twelve words and containing three examples of each letter pattern. Each anagram was typed in block capitals on a $3\frac{1}{2} \times 2\frac{1}{2}$ in. card.

The subjects, 28 naval ratings, were tested individually. They were given three easier practice anagrams, WORDL (World), ASUGR (Sugar), and ENACD (Dance), for which there was no time limit, followed by the test series. The order of presentation of items was randomized for each subject and he was timed with a stopwatch.

If he solved the anagram the time was noted, the card removed and the next item presented. If he failed to solve it within a minute, he was told the solution and the fact noted before removing the card and giving him the next one.

After the last card he was questioned about the strategy he had used. He was then asked to write down as many of the solution words as he could remember in any order he wished.

RESULTS AND DISCUSSION

Subjects solved a mean of 6.46 items and recalled a mean of 4.39 solutions. The percentage recall of solved and unsolved items was calculated for each subject. It was found that subjects recalled a mean of 27.7 per cent. of the items solved and 54.2 per cent. of the unsolved items, giving a mean Zeigarnik interrupted to completed ratio of 1.95. Of the 28 subjects tested, 25 recalled a greater percentage of unsolved than of solved anagrams ($\chi^2 = 17.3$, $p < 0.001$, 2-tail), suggesting a fairly general effect.

The question now arises as to whether this effect is genuinely due to interruption and not to some other factor. Possible explanations can be divided into two categories: those which could be applied to all experiments on the Zeigarnik effect, and those which are peculiar to this situation.

There are at least two possible ways of explaining this result without reference to interruption. First it may simply reflect the fact that the words which make difficult anagrams are also easy to remember, perhaps because of their atypical letter structure. If this were so, the effect would be confined to one or two of the difficult items. A comparison was made for each word of the percentage of correct solutions and the percentage of interrupted solutions that were recalled. Ten of the 12 items were recalled more frequently when interrupted; the difference in favour of interrupted items was significant on a Wilcoxon matched-pairs signed-ranks test ($p < 0.02$, 2-tail), suggesting that the effect is not due to any peculiarities of the words used.

A second possible explanation is that interrupted items are remembered better because the subject spends more time on them than on the items that are solved (Pachauri, 1935). This hypothesis can be tested by looking at the items which were solved. If time spent solving the anagram is important, the items solved fastest should be recalled least often, whereas if interruption is the critical factor, there should be no difference. Each subject's solutions were therefore ranked in order of speed. The mean of the ranks of the items recalled was then compared with the mean rank expected on a chance basis if all solved items were equally likely to be recalled. If a longer amount of time spent solving an anagram increases the probability of recall, then the observed mean rank should be greater than that expected. Of the 28 subjects, 11 had smaller scores and 9 showed no difference, suggesting that solving time is not an important variable.

It seems then that interruption is the crucial factor, though how it facilitates recall is by no means clear either in the present situation or with the Zeigarnik effect in general. The Gestalt explanation, favoured by Zeigarnik herself, suggests that interruption leaves an unresolved goal tension which in turn leads to perseveration of the memory trace and enhanced recall. Two aspects of the present experiment bear on this explanation.

It has been pointed out (Osgood, 1953, p. 584), that if interrupted and completed tasks are similar, this hypothesis would predict that the tension left by the interrupted task should be reduced by the completion of the remaining tasks, causing the Zeigarnik effect to disappear. The tasks in the present experiment are considerably more homogeneous than those used by Zeigarnik, and yet a particularly large effect occurs. The Gestalt hypothesis also suggests that completion of the interrupted tasks by someone other than the subject may prevent the Zeigarnik effect. It might therefore be expected that the experimenter's providing the solution to the interrupted anagrams would resolve the tension and thus nullify the effect of interruption; again this was not the case.

These two factors together suggest that the present result is not based on the sort of mechanism suggested by Zeigarnik. It seems more likely that the crucial aspect of interruption, may be its emotional overtones, which might facilitate recall either because they make the solution more distinctive and less liable to interfere or possibly because such an event is more likely to be rehearsed. However, an explanation of this sort in terms of the side-effects of interruption rather than interruption *per se* could equally well be applied to most demonstrations of the Zeigarnik effect. Until it has been shown that interruption *per se* is the crucial factor it is perhaps wiser to regard the Zeigarnik effect as a purely empirical relationship rather than a demonstration of Gestalt principles.

Anagram solution seems to provide a useful technique for producing a phenomenon which is closely analogous to the Zeigarnik effect. Its advantages are that the material is completely specified, interruption occurs as a natural part of the task, and it requires under 15 min. testing time.

The writer is grateful to the Royal Navy for providing the subjects.

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A REPLY TO JOHN W. SENDERS AND VIRGINIA L. SENDERS

(*Quart. J. exp. Psychol.*, 14, 176-7)

BY

K. GIBBINS and C. I. HOWARTH

From the Department of Psychology, Hull

Senders has put forward a very relevant criticism of our experiments in so far as they are attempts to reproduce her findings. It is fair to say, however, that our primary interest was to discover whether the "Senders' effect" is a common one likely to turn up and confuse the results of any unwary research worker. This it would seem is now obviously *not* the case.

The apparently convincing argument, however, about the effect of fixation does not appear to hold. In February 1960 a simple experiment was carried out to test just this point. The equipment used was that described in Experiment 3 with repetition rate of 5.26 cps., except that:—

The fixation system was a sheet of glass reflecting an illuminated "cross" with the centre removed in which lay the rectangular grill, the subject being told to fixate the point where the centre of the cross *should* be. The open cross was used rather than crossed hairs or a point source as it was thought that these might interfere with the stimulus light at the point of fixation. A chin rest was used and the subject was fitted into a "skin diving mask" from which the face plate had been removed and replaced with a piece of wood with a hole opposite the left eye and the whole of the contraption was attached very firmly to a retort stand, which in turn was clamped to the bench. A cylindrical tube fitted into the hole in the mask and this again was attached to the bench via supports. Into the tube was let in the "fixation" glass reflector.

TABLE I

RELATIONSHIP BETWEEN ENERGY OF A FLASH REQUIRED FOR IT TO BE VISIBLE AS A FUNCTION OF ITS DURATION. 20 ESTIMATIONS PER POINT. SUBJECTS WERE F. RIDLEY AND K. GIBBINS

							Intensity of threshold steady- light
<i>Flash duration</i>							
Millisec. ..	5.00	10.65	20.90	42.90	82.60		
Log. millisec. ..	0.70	1.027	1.320	1.632	1.917		
<i>Line acuity with fixation</i>							
Log. (I.t.)/I ₀ ..	1.865	1.965	2.075	1.965	2.065	F.R.	1.475
	1.99	1.97	2.14	2.19	2.02	K.G.	0.78
<i>Line acuity without fixation (control)</i>							
Log. (I.t.)/I ₀ ..	1.97	1.70	1.81	1.82	2.00	F.R.	1.38
	2.33	2.49	2.37	2.40	2.367	K.G.	0.42
<i>Grating acuity with fixation</i>							
Energy of flash							
Log. (I.t.)/I ₀ ..	1.83	1.717	1.87	1.942	1.947	F.R.	1.83
	2.21	2.24	2.09	2.13	2.11	K.G.	0.14

Standard deviations are all around 0.25 Log. units.

Standard errors are therefore all well less than 0.1 Log. unit.

The results, which seem to support "Bloch's law" are given in Table I. There were twenty estimations per point. Two types of targets were used: the grid one like Senders's optimum one and a single hair approximately 30 sec. of arc across as measured by micrometer at the distance from the eye.

In no case do the estimations differ significantly from each other. The standard deviations of each point are all about a quarter of a Log unit and the standard errors, therefore, are all less than 0.1 Log. units.

It is felt that the reasoning against accepting Gerathewohl and Taylor's data is not completely clear. The opportunity is taken, therefore, of clarifying it.

The reasoning is as follows. Senders's experiment, like ours, has black stripes of constant contrast on a variably bright background. It is the brightness of the background which is measured. In the Gerathewohl and Taylor experiment, however, the contrast decreased as the intensity decreased. Senders has ignored this in her calculations of the Bloch's law prediction. It is easy to show that if Bloch's law holds in a situation where the contrast is constant, it cannot hold in a situation where the contrast varies in the way it does in the Gerathewohl and Taylor experiment. Suppose exposure time is doubled, if Bloch's law holds for a constant contrast situation then:

$$I_1 t_1 = I_2 t_2$$

where $I_1 = 2I_2$ and $2t_1 = t_2$

In the Gerathewohl and Taylor experiment the contrast decreases as I decreases, so that doubling the exposure time will not now compensate for halving the intensity since simultaneously the contrast will have been reduced. So in this case we would expect $I_2 > \frac{1}{2}I_1$ and Bloch's law will not appear to hold.

It is difficult to calculate what the Bloch's law expectation would be when the contrast varies in this way. But if one assumes that Talbot's law holds at frequencies above fusion then the relationship which is found between I and t at above fusion frequency should represent the reciprocity expectation. This is, in fact, what Gerathewohl and Taylor did, and because their results were similar above and below fusion, concluded that Bloch's law was obeyed.

Senders also obtained similar results at above fusion frequencies, which could indicate that in her experiments, the contrast may have varied with the exposure.

The discrepancy between Senders's results and our own is not yet accounted for. We are convinced that eye movements and fixation are not the answer. Some of our recent experiments suggest that Bloch's law holds when the contrast is high but not when the contrast is low. In the latter case, however, the deviations from Bloch's law are in the opposite direction from those found by Senders and are easily understood as a special case of Weber's law.

For each subject under each condition the results are all within chance deviation from each other on the assumption that they are samples from the same population.

The reason *all* data do not approximate only one value, is that they are increased in variability by the variability in assessment of the steady light intensity, which has been subtracted in each case.

APPARATUS

THE RECORDING OF CONTINUOUS VARIATION ALONG FOUR DIMENSIONS

BY

N. F. DIXON

From the Department of Psychology, University College, London

A system is described for signalling, recording and analysing continuous changes in up to four dimensions applicable to the visual after-image and other phenomena. Some advantages of the method are discussed.

INTRODUCTION

A difficulty inherent in the study of after-images and kindred phenomena is that of recording changes in the dependent variable. There are two problems here, relating to (a) the responses given, and (b) the recording of data and drawing of conclusions. For (a) the problem is that of signalling changes in up to four dimensions of the image without using a response system which will itself interfere with image formation. Normal practice is to rely upon verbal reports. These suffer, however, from several disadvantages. If introspective they are inclined to interfere with what is being reported. If retrospective much information is likely to be lost, particularly that relating to the time course of events. Either way the semantic problem of describing, for example, complex colour changes, of communicating continuous variation with a discontinuous language, and of transforming into verbal symbols at an information rate which may be as high as 20 bits/sec. makes verbal report at the best an indifferent measure.

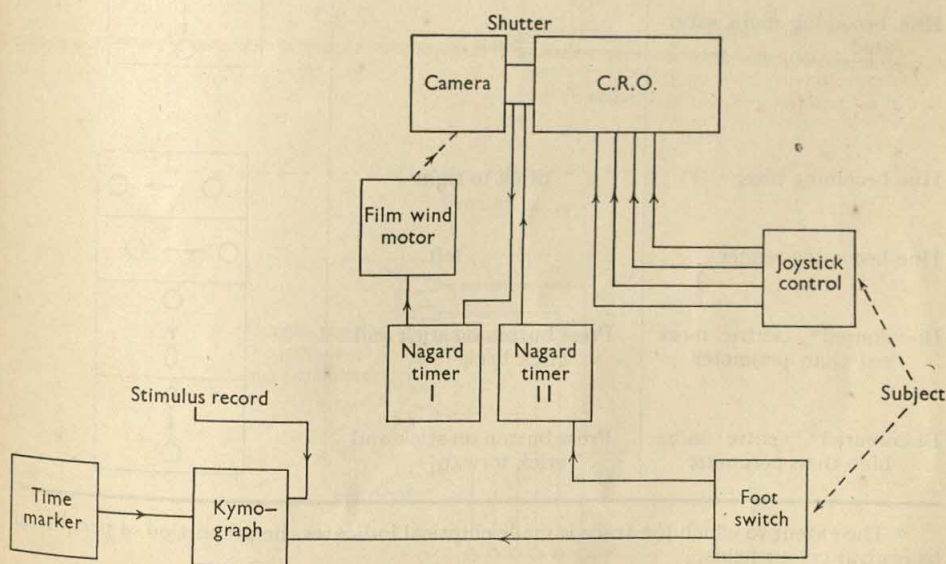


FIGURE 1

On the second problem (b) the experimenter is faced with the equally difficult reverse symbolic process of getting down, and subsequently envisaging, in his own mind, what it was his subject saw.

It was with these considerations in mind that the following apparatus has been devised.

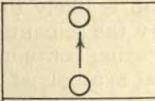
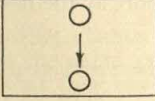
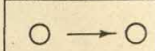
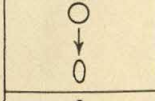
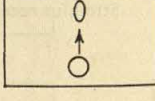
As shown in the sketch, the apparatus consists of a CRO whose trace may be governed for size, shape and orientation by an external joystick control. All changes in the trace are recorded by a camera triggered for single shots from a foot control, which simultaneously produces a record of shots taken against a kymographic time scale.

METHOD OF USE

At present the apparatus is being used to investigate the effect upon the monocular after-image, at various stages of its growth and decay, of intense brief stimulation of the other eye.

A brief description of the procedure for this study will exemplify one way in which the apparatus may be used.

Over a number of sessions the subject is trained to move the joystick according to instructions given by the experimenter. The various movements, image parameters and trace changes are as follows:—

<i>Image</i>	<i>Movement</i>	<i>Trace</i>
Enlarging	Twist grip to right	
Becoming smaller	„ „ „ left	
Hue becoming less saturated	Stick forwards	
Hue becoming more saturated	„ back	
Hue becoming bluer ..	Stick to right	
Hue becoming redder ..	„ „ left	
Bi-coloured* centre more red than perimeter	Press button on stick and stick back	
Bi-coloured* centre more blue than perimeter	Press button on stick and stick forward	

* The extent to which the trace is made elliptical indicates the proportion of peripheral to central colour fields.

When the subject has reached a given criterion of accuracy in following the experimenter's directions, he is further practised on after-images induced by the apparatus shown in Figure III. This consists of a pair of light excluding goggles in which a number of micro stimulators have been inserted. The switching device by which particular stimulators are selected produces a signal on the a/m kymograph record. During this practice period he verbalizes the changes he is attempting to signal.

For the experimental trials the subject is merely required to indicate by the joystick all changes in the monocular after-image (whether these occur before or after intermittent stimulation in differing regions of the visual field of the other eye). With each as it occurs, he sets the four variables of colour, size, saturation and mixture and then, via the foot-switch, photographs the resulting CRO trace.

The data on photographic paper from a given series of trials may be chopped into separate frames and set against the kymograph time record. Large deflections above the time scale may be used to indicate prolonged stimulation of one eye, those below the line brief stimulation of the other eye.

Alternatively, to get an overall impression of changes over time in the after-image, the entire series of frames may be projected using a 35 mm. cine projector.

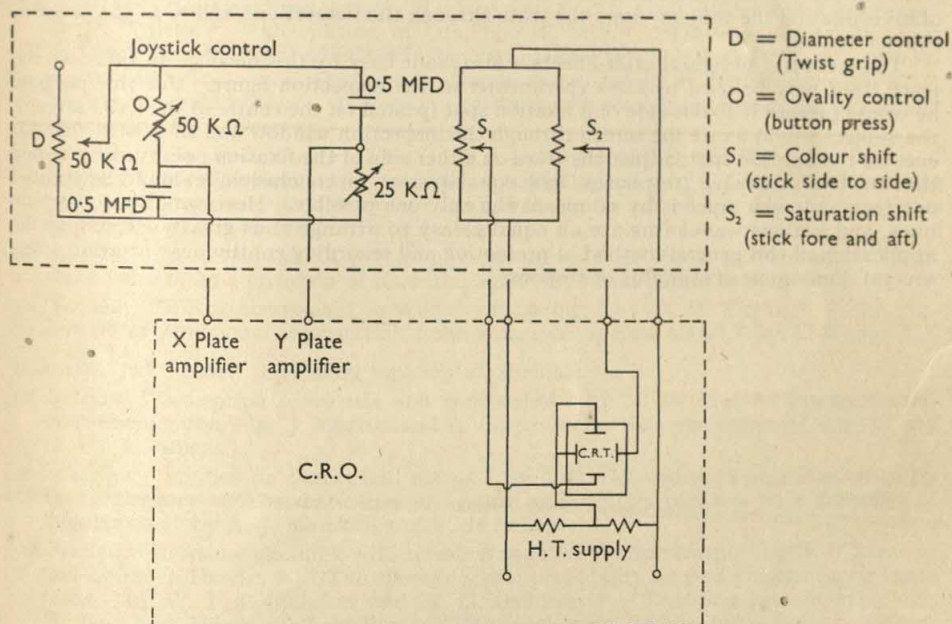


FIGURE 2

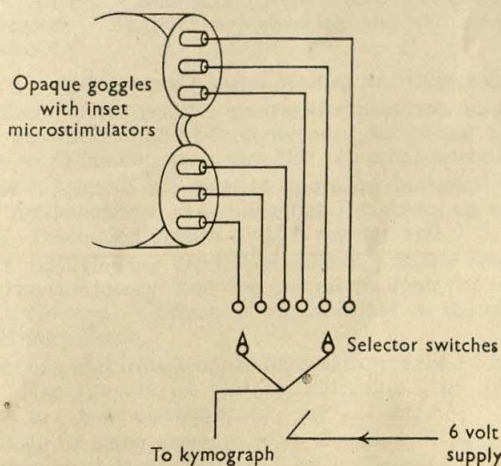


FIGURE 3

OTHER USES

Though designed for the recording of visual after-image data the apparatus may in fact be used for any situation wherein the subject is required to report at any one time upon more than one aspect of his subjective experience. In constancy experiments, for example, the trace might well be used as the variable stimulus patch which the subject adjusts to match the standard. In this context the apparatus makes possible the difficult task of comparing size, shape and brightness constancy, for the same patch, at one and the same time. For such an experiment the CRO trace constitutes the variable test patch which the subject adjusts for size, shape and brightness to match the standard at a given distance orientation and luminance. (It is a simple matter to make fore and aft movements of the joystick control brightness rather than position of the trace.) For this sort of investigation the subject views the trace through the inspection window in the camera-shutter tunnel.

The recording of figural after-effects is also made easy by this means. In this case the trace itself may be used by the experimenter as the inspection figure. For this purpose he would present it to one side of a fixation spot (pasted on the centre of the CRO screen), the subject would fixate the screen through the inspection window and then, still with his eyes on the fixation spot, adjust the trace on either side of the fixation point until satisfied that the two successive "responses" look equal in size. In conclusion, it should be pointed out that a circular trace is by no means the only one possible. Horizontal lines, vertical lines, and various waveforms are all equally easy to arrange thus greatly extending the application of this general method of presenting and recording continuous variation along several dimensions of stimulus or response.

PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY SOCIETY, 1962

2nd-4th April, 1962. Extended meeting at Cambridge.

1st Session: "Occipital lobectomy and contour discrimination in the monkey," by L. Weiskrantz. "Tactile discrimination of shape by octopus," by M. J. Wells.* "The effect of drugs on the maintenance of a rhythmic response in a moth," by D. Blest and T. Collett.* "The influence of two tranquilizers on human performance in a serial responding task, with some additional observations on blockings," by P. Bertelson and Rachel Joffe.*

2nd Session: "Movement perception," by S. Anstis.* "Retinal rivalry and brightness," by P. Whittle.* "Alternation in rats," by A. Still.* "The size weight illusion," by Helen Ross.* "Individual vocabulary word currency," by R. C. Oldfield. "Working models of a semicircular canal," demonstrated by D. H. Thompson and W. J. Tonkins, introduced by A. J. Benson. A short talk concerning apparatus on demonstration by R. L. Gregory.

3rd Session: "Open and closed Problems," by B. Babington Smith and P. Delin. "Physiological and psychological predictors of the outcome of ECT" by T. Gonda.* "Electrical activity of single cells in the visual cortex of the unrestrained cat, and their response to photic and somatic stimuli," by G. Horn.

4th Session: "On AX and ABX psycho-physical procedures," by I. M. Hughes.* "Constant velocity as a criterion of muscular skill," by Anna Zajackowska.*

5th Session: "GSR measurement in studies of driving," by D. H. Taylor.* "The perception of directional information (with reference to road signs)," by J. Walker.*

16th-18th July, 1962. Extended meeting at Sheffield.

1st Session: "Perception of vowels and consonants," by D. B. Fry.* "Formants and vowel perception," by J. Morton and A. Carpenter. "The perception of speech," by D. E. Broadbent.

2nd Session: "Studies on perceptual norms," by I. P. Howard. "Visual illusions," by R. L. Gregory. "The distortion of Hering and Wundt parallels as a function of angular size," by A. J. Marshall and V. de Lollo.*

3rd Session: "Response chaining with terminal negative reinforcement," by D. P. Hendry and Louise S. Hendry.* "The effect of signal probability on performance in vigilance tasks," by W. P. Colquhoun and A. D. Baddeley.* "Learning sets in rats," by H. Kay and Hilary Oldfield-Box. "The 'forgetting' of the following response in domestic chicks," by K. J. Connolly.

4th Session: "Decision taking under risk," by N. Moray. "Verbal learning and proactive inhibition," by P. Warr.* "Adaptation level effects in response to taste mixtures," by R. A. M. Gregson. "Span of apprehension and the confusion function," by Everdina A. Lawson.*

2nd-4th January, 1963. 15th Annual General Meeting at Birkbeck College, London.

1st Session: "The effects of instruction on transfer processes in imbecile and normal children," by P. Bryant.* "Short-term memory for paired associates," by B. B. Murdock.* "The word-in-context: a new test of verbal reasoning," by Alice Heim.

2nd Session: "On the subjective hardness of compliant materials," by R. Harper and S. S. Stevens. "Achievements and difficulties in measuring sensory magnitudes," by E. C. Poulton. Discussion opened by M. Treisman and S. S. Stevens.

3rd Session: "Object and viewing conditions affecting shape constancy," by W. H. Gladstone.* "'Over-constancy' and the central tendency of judgment," by R. B. Joynson and L. G. Newson. "Effects of ethyl alcohol on the action potential of the human eye," by Hisako Ikeda.

4th Session: "The effect of a barbiturate on the habituation of the PGR in normal subjects," by M. Lader.* "Experiments on GSR conditioning," by Irene Martin.* "The heart rate response to a brief electric shock," by J. Brener.*

5th Session: "Physiology of sleep accompanying dreaming," by I. Oswald. "The experimental modification of dream content," by R. Berger.*

* Speakers by invitation.

Committee, 1963: President: Professor O. L. Zangwill.

Editor: Professor O. L. Zangwill.

Ordinary Members of the Committee: Dr. R. J. Audley (Hon. Secretary), Dr. J. Brown, Dr. R. Davis (Hon. Treasurer), Mr. R. L. Gregory, Dr. N. Moray, Dr. J. Symons, Dr. M. Treisman, Dr. P. Venables (Asst. Hon. Secretary), Dr. L. Weiskrantz.

The following have been elected into membership of the Society:

- A. J. Benson, M.B., Ch.B., Neurobiology Section, R.A.F. Institute of Aviation Medicine, Farnborough, Hants.
- W. P. Colquhoun, Ph.D., M.R.C., Applied Psychology Research Unit, 15 Chaucer Road, Cambridge.
- A. Cowey, Ph.D., Psychological Laboratory, Downing Street, Cambridge.
- H. C. A. D  le, Ph.D., M.R.C. Applied Psychology Research Unit, 15 Chaucer Road, Cambridge.
- K. Danziger, M.Sc., D.Phil., Department of Psychology, University of Cape Town, South Africa.
- Frieda Goldman-Eisler, D.Phil., Department of Phonetics, University College London, Gower Street, W.C.1.
- L. J. Herberg, M.B., B.Ch. (Rand), Institute of Experimental Psychology, Oxford.
- M. Kinsbourne, B.M., B.Ch. (Oxon.), Radcliffe Infirmary, Oxford.
- D. Legge, B.Sc., Department of Psychology, University College London, Gower Street, W. C.1.
- B. N. Lewis, B.Sc., System Research, 20 Hill Rise, Richmond, Surrey.
- J. Morton, Ph.D., M.R.C., Applied Psychology Research Unit, 15 Chaucer Road, Cambridge.
- N. Mrosovsky, Ph.D., M.R.C., Unit for the Experimental Investigation of Behaviour, Department of Psychology, University College London, Gower Street, W.C.1.
- W. R. A. Muntz, D.Phil., Magdalen College, Oxford.
- J. M. Oxbury, Ph.D., London Hospital, Medical School, London, E.1.
- P. M. A. Rabbitt, Ph.D., M.R.C., Applied Psychology Research Unit, 15 Chaucer Road, Cambridge.
- I. S. Russell, Ph.D., M.R.C., Unit for the Experimental Investigation of Behaviour, Department of Psychology, University College London, Gower Street, W.C.1.
- G. K. Wallace, Ph.D., Department of Psychology, University of Reading.

Visiting Foreign Members

- T. Gonda, Ph.D., Institute of Experimental Psychology, Oxford.
- A. J. Marshall, Ph.D., Department of Psychology, University College London, Gower Street, London, W.C.1.
- D. Lindsley, Ph.D., Psychological Laboratory, Downing Street, Cambridge.
- B. B. Murdock, Ph.D., Applied Psychology Research Unit, 15 Chaucer Road, Cambridge.

PUBLICATIONS RECEIVED

JOURNALS

- Studia Phonologica*. No. 1, 1961. Edited by Hisanosuke Izui. Report of the Institution for Phonetic Sciences, University of Kyoto, Japan. (English and Japanese text.)
- Bulletin of Art Therapy: Art in Education, Rehabilitation and Psychotherapy*. Vol. 1, No. 1. Fall 1961. Edited by Elinor Ulman. Editorial and Subscription Offices: 634A Street, S.E., Washington 3, D.C. Issued four times a year. Subscriptions: U.S. and Possessions, \$3.00 a year; Foreign, \$3.50. Single copies \$1.00.
- dsh Abstracts*. Vol. 11, No. 3. July, 1962. Edited by Jerome D. Schein, Ph.D. Published by the Interstate Printers and Publishers, Inc., 19-27 North Jackson Street, Danville, Illinois. Issued four times a year. Subscriptions: U.S. \$8.00 a year. Foreign \$8.50. Single copies \$2.10.
- Manas* 1961. Edited by Prayag Mehta, J. M. Ojha and Udai Pareek. 32, Faiz Bazar, Delhi 6. Published twice a year. Annual subscription \$4.50 for foreign subscribers. Concessional subscription rates to members of Psychological Societies.
- Culegere de Studii de Psihologie*. Vol. IV. 1962. Edited by Adriana Oancea. Published by Institutul de Psihologie, Academia Republicii Populare Romine, Bucharest.
- Forja-Briefe Beitrage zur Betriebs- und Arbeitspsychologie*. Edited by Prof. Herwig Braunschweig. 2-monthly. Annual Subscription DM 43.20. Single numbers DM 7.20.

BOOKS AND OTHER PUBLICATIONS

Review in a later issue is not precluded by notice here

- The Brain as a Computer*. By F. H. George. International Series of Monographs on Pure* and Applied Biology. Zoology Division, Volume 8. Oxford. Pergamon. 1961. Pp. 413. 63s.
- On the Diversity of Morals*. By Morris Ginsberg. London. Heinemann—Mercury Books. 1962. Pp. xiv + 329. 12s. 6d.
- The Psychology of Jung: A Critical Interpretation*. By Avis M. Dry. London. Methuen's Manuals of Modern Psychology. 1962. Pp. xiv + 329. 35s.
- Fundamental Physical Theory and the Concept of Consciousness*. By Johan H. Greidanus. Oxford. Pergamon. 1961. Pp. xi + 159. 30s.
- Secretory Mechanisms of the Gastro-intestinal Tract*. By R. A. Gregory. Physiological Society Monograph. London. Edward Arnold. 1962. Pp. vii + 248. 36s.
- Action for Mental Health*. Final Report of the Joint Commission on Mental Illness and Health, 1961. New York and London. Wiley. 1962. Pp. xxxviii + 338. 15s.
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BOOK REVIEWS

Annual Review of Psychology, Vol. 13. Edited by Paul R. Farnsworth, Olga McNemar and Quinn McNemar. Annual Reviews, Inc. Palo Alto, California. 1962. Pp. 602.

This year's *Annual Review of Psychology* is as unlovely and invaluable a book as usual. But an innovation is the inclusion of a chapter on Consumer Analysis. The author, Lester Guest, restricts himself to discussing studies that "have some unique method or content," though this makes what follows a little dissonant. For example we learn that in England "Ten years of study have been devoted to the sensory assessment of the eating qualities of white fish" and from this some test panel techniques have been devised. To compare this to the work on foodstuffs in other countries may set psychologists reflecting on the way different social structures influence the development of their discipline. In Russia psychologists are more concerned with the effects of various diets on the functioning of the central nervous system than their sensory qualities. This emerges from Brozek's survey of Russian work—extensive and well organized as it is the reader will still feel as if he is looking at the top of an iceberg—in which as might be expected there is no mention at all of consumption.

The restrictions of space in the *Annual Review* inevitably mean that evaluation by the authors of the studies they cite is often too laconic to be informative. But despite all difficulties judicious and readable articles appear such as those on Hearing (Weaver), the Somesthetic Senses (Nafe and Kenshalo) and Educational Psychology (Warburton). A table summarizing the results of television versus conventional teaching by Schramm in his paper on Mass Communication (also a new topic covered this year) shows how a simple overall impression can be given economically in a area where there are many diverse results.

However, space is not always attractively used. To be reprobated are the esoteric humour and uninstructional light writing that are found in some of the papers. The editors should discourage sub-standard passages like: "(Confidentially, in approaching this titillating adventure, the reviewer finds himself obsessed by the plight of the Maine logger who was reported to have said, 'Saturday night I'm going to Bangor and get drunk, but, Lord, how I hate the thought of it!')"

To what extent discussions of methodology should be included in the *Annual Review* is another problem facing the editors. Reitan's paper on Psychological Deficit is almost entirely devoted to sound necessary cautions about work involving brain lesions and apart from this there are considerable sections on methodology in a number of the other reviews. General appraisals, similar to those too frequently heard at conferences, usually lack power unless thickly enriched by the quotation of mistakes from the literature. It is discussions of specific technical matters, such as those in Lubin's article on Statistics on treatment carryover and what has been called the "problem of the premature plateau," that are needed. In the review on the Neurophysiology of Learning (Thomas) doubts about interpretations from ablation studies are briefly cited, without making the reader who wants to look up the latest experiments impatient, and the author then goes on to summarize recent substantive results. These in themselves illustrate more general points like the remarkable differences between single and multistage lesions. It is where such delicate balance between experimental results and comment, constructed out of the recent materials, has been achieved that the *Annual Review* is at its most successful and useful to teachers and research workers.

N. MROSOVSKY.

The Structural Basis of Behavior. By J. A. Deutsch. The University of Chicago Press. 1960. Pp. viii + 186. \$3.50.

Deutsch argues that psychology has in recent years largely concerned itself with fact-gathering and statistics, and made little attempt to co-ordinate its data into a theoretical system. The time has now come, he asserts, for us to give a coherent explanation of some of the evidence experimental psychologists have amassed in the fields of motivation, learning, reasoning and perception.

As a preliminary he examines the nature of explanation itself. He finds two main types (1) descriptive or generalizatory, (2) structural or mechanical. (1) is basically that of a particular instance subsumed under a general statement. In (2) the event is explained as being deduced as a property of a structure, system, or mechanism. To illustrate the

the difference between (1) and (2) he compares Boyle's Law with the kinetic theory of gases. Whereas the former is a general statement about the observed causal connections between the temperature, pressure and volume of a gas, the latter explains these facts by postulating that gases have a certain structure. From this assumption follows the hypothesis concerning the volume of the gas. Similarly, in the case of animal behaviour, an explanation may be sought by specifying the mechanism or structure which produces its behaviour. If we knew what this was, explanations of type (1) would be shown to be derivable from those of (2). It would be possible to deduce logically the behaviour of a mechanism whose structure we knew.

Although causal explanation is confined to the early stages of science at the level of organized common sense, it is doubtful whether psychology has gone much beyond this stage.⁶ It is true that in a more advanced science such as physics, we can introduce abstract models; perfect fluids, ideal gases, etc. These enable us to understand the mode of connection between the facts we are trying to explain. In Deutsch's enthusiasm for models, he overlooks that in modern quantum theory statistical laws which are generalizations from experience, have tended to supersede the older structural models of classical mechanics. Such laws also play an important part in psychology. Indeed the very nature of psychological reality may be such that statistical laws of behaviour are the most we can hope to achieve.

Further, the above example is not so much the replacement of a causal by a structural explanation, as the subsuming of a restricted theory under a more general one. Boyle's Law is not counted as an experimental law of classical mechanics. It is, however, shown to follow from the principles of mechanics, when these are supplemented by a hypothesis that a gas is composed of molecules whose motion conforms to these principles, a statistical assumption concerning the motion of the molecules, and a postulate connecting temperature with the mean kinetic energy of the molecules. It is therefore difficult to see how this example helps Deutsch, since the hypothesis of structure is here really subsidiary to that of statistical explanation.

Deutsch believes we could abstract the structure of our behaviour from its particular concrete embodiment, and that such a system can give us important and enlightening information. The precise properties of the parts, he tells us, do not matter, it is only their general relationships to each other which gives the system as a whole its behavioural properties. This system, he believes, can be put into a perfectly precise non-numerical form by means of Boolean algebra. It could then be embodied in a machine to exemplify the same behaviour as animals, namely, memory, insight and learning.

The formal type of model advocated by Deutsch differs from the more pictorial models used in science, as it is a system of abstract relations of the sort we meet with in logic and group theory. However, this approach overlooks that unlike the natural sciences, one of our tasks in psychology and the human sciences in general, may be to seek complexity in the shape of qualitative detail, rather than simplicity of structure. Deutsch conceives some form of isomorphism between psychological and physiological structures. In this way, he hopes, the two fields of enquiry may become truly complementary. However, to say that two systems have the same structure may be quite trivial, unless the related properties have also certain other characteristics in common.

The view that information about our behaviour and nervous system can be put into a precise form by Boolean algebra, dates Deutsch's model to the first flush of enthusiasm for logical models of the brain. It was then assumed that all intelligent behaviour was based on machinery which in organisms possessing a nervous system, operated on the principle of digital computers. Nowadays cyberneticians would draw their analogies more from probability theory, and would tend to emphasize the random character of brain activity. Our behaviour may not manifest itself in any precise way, logically deducible from some clear-cut set of principles. It may only be predictable in probability or statistical terms.

Deutsch works out this notion of structure in his mechanism of needs. He postulates a set of elements (primary links) somewhere in the nervous system. These are set in activity by the environment, as is the case in hunger, thirst, mating, etc. Each element is connected to a receptor which when set in activity indirectly excites the motor system. The resultant behaviour produces a particular environmental change. This in turn stimulates the receptor, depressing its sensitivity, and thus terminates the element's activity. In opposition then to Hull, Tinbergen and Lorenz, Deutsch argues that stimulation does not set off activity, but switches it off.

A machine working by means of relays could, he tells us, if it were constructed on these lines, also manifest goal-seeking behaviour. The described properties are not those of relays, but of an arrangement of relays. During "learning," for example, the relays (receptors) are arranged in a series inverse to the order in which they were stimulated, so that the goal-signalling relay will be excited first. Consequently on a subsequent occasion when the machine is made to run the maze, it will steer towards the goal. Not only does Deutsch claim that this device can "learn" a maze, but he also believes that it is capable of "reasoning" and "insight."

Deutsch assumes that each need has a localised element in our nervous system corresponding to it. Although he claims some physiological evidence for this assumption, his account is oddly reminiscent of the Aristotelian theory of instincts. The whole conception of a need may, however, be far from simple. Instead of referring to one and only one classificatory element, it may be merely a short-hand expression for a complex of functional relationships. The somewhat novel and paradoxical feature of this theory, that activity switches off excitation, seems to be really a reformulation in terms of switching circuits of the Weber-Fechner Law, as well as the law of diminishing returns. It is another way of saying that in assessing the present performance of a behavioural system, we have to take into account its past history.

Deutsch's theory of needs would seem to arise from his attempt to identify the operations of our nervous system, with the disjunctive and conjunctive arrangements of relays and switching devices. Although we can set up the machine so that the arrangement of its relays comes to match that of the geometrical pattern of the maze, we are not justified in assuming that the neural structures underlying animal learning exemplify similar properties. Because two systems exhibit the same sort of behaviour, it does not follow that they have been produced by a similar mechanism.

Deutsch is highly critical of the use of ablation techniques in animal psychology, where we correlate the geography of lesions with the symptomatology of behaviour. Such behaviour, he argues, is of a lower grade of evidence than that displayed by the intact animal, and is open to many more interpretations. He does admit, however, that we can in this way discover that separate systems are responsible for carrying out a complex function. In man, for example, aphasia due to lesions may throw light on our complex sensory and conceptual processes. Deutsch's objections to ablation studies ought also logically to apply to the use in psychology of abnormal data from psycho-pathology and mental defect. Where would personality studies, however, be today without the work of Freud, or the study of intelligence without Binet's pioneering work among the mentally defective? Analogies drawn from communication engineering and digital computers seem to be inadequate when we come to study the brain. These kinds of model do not take account of the great resilience of the nervous system under the effects of widespread injury.

Deutsch's account of space-perception is mainly concerned with the perceptual mechanisms of the lower animals, such as the rat. He proposes a mechanical system which will abstract form and suggests that shape-recognition is effected by the conversion of a spatial extent into a temporal one. Once again the use of analogies from communication engineering may cloud rather than clarify. Why must the spatial shape need to be transformed into a temporal sequence as is the case in television? Is it because this sort of electronic device functions essentially in an atomic way, and will not imitate the behaviour of the brain if it is taken to work on Gestalt principles? Even if we reject a Gestalt approach, there may be an enormous net-work of lines available in the brain, so that the whole pattern is transmitted along parallel lines of nerves, rather than needing to be transformed into a temporal sequence. There also seems to be little overlap between Deutsch's views on learning and perception. This is somewhat curious, as shape-perception is a subject not entirely divorced from the problem of learning.

Deutsch's book shows a very ingenious and theoretical mind at work, but one also conscious of the need for the experimental verification of his theories. He is aware more than most psychologists of the necessity for discussing the nature of psychological explanation. It is doubtful, however, whether his logical and mechanical models are adequate to account for the great flexibility of human and animal behaviour. Psychology has still a great deal of fact-gathering to do before it is justified in putting its data into axiomatic form. Further, it is possible that we may not be able to disengage all the relevant variables underlying our behaviour. We may therefore have Hobson's Choice; our explanations may have to be of a statistical rather than of a structural type.

W. MAYS.

The Nature of Sleep. Ciba Foundation Symposium. Edited by G. E. W. Wolstenholme and M. O'Connor. London. Churchill. Pp. xii + 416. 1961. 50s. net.

This volume contains 18 papers read to an authoritative gathering by many of the well-known experts in the field of sleep. In the accepted manner of such symposia, each paper is followed by a discussion, recorded verbatim. This is an excellent procedure since the discussions are often more interesting than the papers themselves. (This remark is meant to praise the level of discussion and not to detract from the worth of the papers, which are in the main first class.)

The order in which the papers were presented could have been better, but roughly the first half of the volume is taken up with reports of research of an extremely neurophysiological nature, while the second half touches fields of wider interest, from hibernation to the sleeping behaviour of men in the Arctic. The symposium concludes with a final discussion which is rather disappointing.

The neurophysiological experiments reported are largely concerned with whether sleep is an active or a passive process. Bremer, who is alone in maintaining that active inhibitory phenomena are not necessary either to account for sleep or for certain microelectrode observations ostensibly in support of inhibitory explanations, presents a paper which is praiseworthy, if only in demonstrating its author's love of parsimony. Yet parsimony can be carried too far, and when phenomena arise which stretch the simpler explanation beyond its limits, it becomes proper to choose the more complex. This step, it would seem, ought to be taken if we judge from the facts reported by Dell, Bonvallet and Hugelin, Verzeano and Negishi, Creutzfeldt and Jung, Evarts and Jouvet, which virtually insist on an inhibitory explanation. We find ourselves once again meeting in these papers a refined, but easily recognizable Pavlov.

We are told that cortical inhibitory and excitatory processes act reciprocally, that the Brain Stem Reticular Formation must now be considered to have inhibitory as well as facilitative effects on the classical sensori-motor pathways, that it responds inhibitory under some conditions to ascending as well as descending innervation and is involved in various feed-back loops in such a way that by its own facilitative effect it can indirectly bring about its own inhibition. Sleep and wakefulness are apparently not easily differentiable in terms of gross neuronal discharge rates, yet interesting differences between the two states do seem to exist. Evarts, for example, suggests that the ratio of elicited to spontaneous activity (signal to noise ratio) is increased in the waking state. Differences between light and deep sleep are emphasized in several examples by Verzeano and Negishi, the former showing patterns suggestive of arousal levels above those of the waking state. Faced with these complications the reader is hardly surprised to learn from Kety in the latter half of the symposium that no differences in total brain metabolism exist between sleep and wakefulness, but one ought still to be rightly puzzled in regard to the obvious recuperative effects of sleep.

The work reported in the latter half of the volume was more behaviourally orientated and served to stimulate more varied discussion. The simple experiment reported by Gastaut and Bert on habituation to auditory stimulation, for example, resulted in the lengthiest and most interesting discussion. General interest will be stimulated by the papers of Rowland, Suomalainen, Lewis, Wilkinson and Kleitman.

Sir John Eccles in his closing remarks comes straight to his point: problems for future investigation. This is the real worth of this symposium and is recommended reading for psychologists and physiologists for this reason.

D. CORCORAN.

The Machinery of the Body. By Anton J. Carlson, Victor Johnson and H. Mead Cavert. 5th Edition, revised and enlarged. University of Chicago Press. 1961. Pp. xix + 752. \$6.50.

In spite of its popular title, this is a solid intermediate text-book of physiology which presupposes knowledge of biology to about 1st M.B. level. Its scope lies somewhere between Winton and Bayliss and Samson Wright and it has features in common with both these well-known British texts, though the presentation is very much in contemporary American style. The book might be of use to students of psychology who seek firmer grounding in physiology than is provided by the skeletal physiology of the nervous system, senses and endocrine glands traditionally included in psychological texts.

O. L. ZANGWILL.

Persuasion and Healing. A Comparative Study of Psychotherapy. By Jerome D. Frank. London. Oxford University Press. 1961. Pp. xiv + 282. 35s.

The image of psychotherapy which seems to have emerged from the recent wrangles of the more extreme of psychologists and psychotherapists is a caricature, but one which has been difficult to correct because of the lack of a suitable text. Its grotesque features should be removed by the sensitive and urbane account now provided by Dr. Frank, one of the leaders of the younger generation of eclectic psychotherapists.

Dr. Frank regards psychotherapy as essentially the arousal and display of attitudes under conditions in which they can be influenced, i.e. in which learning can take place. Mental illness is due to failure to adapt to stress because of obstacles to learning or disorders in the learning process. Psychotherapy produces and maintains the degree of arousal optimal for learning, combats despair, insecurity and suspicion, gives more accurate understanding of the problems and ways of dealing with them, and encourages participation in activity that leads to behavioural change outside the therapeutic situation. It can conveniently be classified as either directive or evocative.

Dr. Frank claims no special position for any professional group, and is at pains to point out the similarities as well as the differences in the social roles and methods of shamans, charlatans, priests, evangelists, psychiatrists, psychologists, social workers and thought reformers. These are the persuaders. On the other hand are the sufferers: invalids, penitents and prisoners.

D. RUSSELL DAVIS.

Bird Song—The Biology of Vocal Communication and Expression in Birds. By W. H. Thorpe. Cambridge Monographs in Experimental Biology, 12. London. Cambridge University Press. 1961. Pp. xii + 143, 65 text figures. 20s.

In his Preface Dr. Thorpe points to the revolution in the study of bird song which has taken place during the last 20 years, owing to the development of high quality recording and the spectrographic analysis of sounds. He is concerned mainly with surveying recent developments in the study of bird sounds which have resulted from these modern techniques, but the book really ranges much more widely than this and presents a most satisfactory contrast between the older type of work, terribly hampered by a lack of suitable notation and the recent, much more detailed analysis made possible by the use of sound spectrograms.

After a note explaining how the sound spectrogram provides a visual presentation of the frequency, duration and amplitude of a given sound, the book opens with a discussion of bird song as music, as a means of communication and as language. Next, bird utterances are divided into the two categories of song, and call-notes. This distinction is difficult to maintain, as the author points out, although he is able to discuss song mainly in connection with the reproductive cycle and call-notes mainly with co-ordination of behaviour with other members of the species in maintenance activities. The function of the various call-notes is considered in detail, and it is suggested that these are by no means always innately fixed as to their meaning; for example there are certain calls which appear to express fear, or a tendency to flee, but are also used much more widely than this.

The relation between the structure of call-notes and song is then discussed, showing how the latter is largely comprised of the former, and the territorial nature of song is considered, an account being given of internal and external factors controlling the seasonal and daily periodicity of bird-song.

The most impressive part of the book, however, is that concerned with the author's own work on the development of song in the individual. This shows that the ability to perform the basic outline of their song is innate in the chaffinch while the finer details are individually acquired from other members of the species during the first year of life. A necessary limitation on their ability to copy has been demonstrated in that they can learn a reversed chaffinch song, but nothing which lacks chaffinch tonal quality. Auditory imitation is then discussed, and song learning considered in terms of imprinting; musical invention is regarded as one possible way of explaining song variation between individuals. In the final chapter the anatomical basis of sound production and hearing in birds is described and their vocal performance and sensory capacity compared with that of man.

The book, in fact, covers a very wide field and draws together a great mass of information which should be of great value to those oriented towards the study of bird-song; innumerable problems are mentioned and areas are described where existing conclusions are confusing. For example it is tempting to suggest that where the number of "words" in use as call-notes in several well studied passerine species works out as the same (about 14) for each species, and where there is difficulty in limiting to fear-provoking situations calls usually regarded as expressing fear, some of the confusion could have arisen from a subjective approach in the older type of work. In contrast, Dr. Thorpe's own work has shown how invaluable a particular problem can now be picked out from the mass of information available, analysed with the aid of the sound spectrograph, and, indeed solved.

There is a full bibliography, and two indexes.

M. A. VINCE.

The Biology of Art: A Study of the Picture-Making Behaviour of the Great Apes and its Relationship to Human Art. By Desmond Morris. London. Methuen. 1962. Pp. 176 (with 13 colour plates, 38 monochrome plates, and 57 figures). 36s.

Dr. Morris deals with his subject in a straightforward and comprehensive way. That a book of this moderate size should deserve to be called comprehensive is partly due to the relatively few careful and prolonged experiments which have so far been made (in any country) on picture-making in the primates. Paintings or drawings have been reported from 32 animals (23 chimpanzees, four capuchins, three orang-utans, and two gorillas); but of these there are only three chimpanzees ("Alpha" in Florida, "Jonny" in Vienna, and "Congo" in London) who have a hundred or more recorded works to their credit. Dr. Morris assembles and discusses the evidence, with excellent photographs of some of the finished pictures, and (what is equally amusing and instructive) of some of the artists at work.

Once an ape has learnt that it can make marks on a card with a pencil or paint brush it develops progressively (in successive sessions) from more or less random squiggles to quite complex and relatively organized patterns. The colours chosen, as well as the designs, are often attractive to human eyes. So far no ape has painted what can be recognized as a picture of anything (it stops just infuriatingly short of the representational stage); but up to this stage its drawings have, as Dr. Morris points out, many similarities with those of a child.

An interesting point is the extreme earnestness with which an ape sets about its work, refusing food and resenting any interruption; and then, when the picture is finished to its own satisfaction, refusing to add another line. Attempts to distract the artist when at work, or to coax him to continue when he has finished both lead merely to temper tantrums.

These ape pictures provide important material for students of the origin of art, and they, as well as students of animal behaviour, will be grateful to Dr. Morris for this fascinating book.

G. C. GRINDLEY.

Measuring Human Motivation. Edited by R. C. Birney and R. C. Teevan. 1962. Pp. ix + 181. 12s.

Emotion: Bodily Change. Edited by D. K. Candland. 1962. Pp. viii + 263. 15s.

Insight Books Nos. 6 and 7. Van Nostrand. Princeton, New Jersey, New York and London.

These two volumes of selected readings have been prepared specially for inclusion in this paperback series. The first contains extracts from Freud, Jung and Allport in addition to more conventional experimental reports, e.g. Janet A. Taylor's well-known study of anxiety and the conditioned blink response. The second contains extracts from James (though not Lange), Watson (though not Cannon), as well as reprints of a number of comparatively recent experimental papers, including Brady's rather horrifying account of the experimental production of gastric ulcers in monkeys. Although this selection is fairly representative on the psychological side, it is distinctly weak in the area of psychosomatic medicine. Both volumes, especially the second, will be of value to students and are on the whole good of their kind.

O. L. ZANGWILL.

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Part 2

THE BASIS OF A METHOD OF PERIMETRY WITH MONKEYS

BY

ALAN COWEY

From the Psychological Laboratory, University of Cambridge

The value of perimetry as a means of studying the visual system is stressed. Perimetry has never been done with animals because it is difficult to record and control their fixation. Existing methods of determining eye position cannot readily be used with monkeys, chiefly because the subject's head must be prevented from moving more than fractions of a millimetre. Other limitations are mentioned. A new method of determining fixation is described. Although not nearly as precise as other methods it is unaffected by head movements as great as $\frac{1}{2}$ in.; it is straightforward to restrict a monkey's head movements to this amount. The eye position is found from the relative positions of four corneal reflections and the borders of the iris. These relative positions change very much more after an eye rotation than they do after a head movement in which fixation has been maintained.

INTRODUCTION

The perimeter is one of the most powerful tools used to analyse the visual system of man. It is a simple instrument which, when used with human beings, enables the experimenter to present a visual stimulus at any point in a subject's visual field. By means of the perimeter, aspects of vision such as acuity, sensitivity, extent of the colour fields, flicker fusion thresholds, and response to movement have been mapped in detail. The perimetric mapping of these functions in human beings whose visual system has been damaged in some way provides clues as to how that system is organized anatomically and functionally. This contribution has been recognized by Polyak (1957, p. 153), who links the invention of the perimeter by Förster in 1867 with that of the ophthalmoscope by Helmholtz in 1851 as the events which marked the beginning of the modern period of investigation of the visual system.

Unfortunately man is far from ideal as a subject for experiments on the effects of brain damage on vision. Brain damage in human beings is rarely completely restricted to appropriate regions; it is usually uncertain that a particular area is totally functionless unless it is removed surgically; and post-mortem evidence of the precise locus and nature of the area in question is seldom obtained. These disadvantages are largely responsible for the slow progress which has been made in understanding the arrangement and mode of action of the visual pathways in man.

These particular limitations are virtually absent in experiments on animals. In such experiments a brain lesion can be placed in the most desirable position and in a manner which prevents or reduces diffuse effects away from the primary site of investigation, and post-mortem histological analysis can be a standard procedure. Yet owing to the absence of a technique of perimetry with animals we know even less about the precise effects of cerebral damage on vision than we do with man, and there

is still no clear-cut evidence pertinent to some of the simplest questions that can be raised about vision. For instance it has not been convincingly shown that unilateral occipital lobectomy in monkeys leads to permanent homonymous hemianopia, i.e. total blindness affecting one half of the visual field.

Perimetry with monkeys has never been done because no one has succeeded in instructing a monkey to fixate, and demonstrated that the animal has obeyed the instruction. Instead of insisting that the animal should fixate a given point it seemed more profitable to discover where he is looking when a visual stimulus is presented. This requires a method of determining eye position.

There are several ways of finding the locus of fixation with human subjects and numerous studies which testify to their accuracy and validity (e.g. Dodge and Cline, 1901; Dodge, 1907; Miles, 1939a; Hoffman, Wellman and Carmichael, 1939; Tinker, 1946; Hartridge and Thomson, 1948). Very often they are subsidiary to a study of eye movements. They fall naturally into three types—the "Dodge" corneal reflection method (so called here to distinguish it from the proposed new method which also uses corneal reflections), electro-oculography, and the use of an attachment to the eye. These three systems are briefly discussed in order to show why they cannot readily be used with animals despite assertions that they may (e.g. Halstead, 1938).

The "Dodge" technique relies upon the fact that an image from a convex spherical mirror, whose axis of rotation lies behind its centre of curvature, appears to move in the direction of the mirror's rotation. The central portion of the cornea resembles such a mirror and a reflected highlight from it (the first Purkinje image) can be photographed. The position of the eyes can be found from the position on the film of the corneal reflection. Exceedingly fine estimates of the fixation point have been made and the errors are said to be less than 10 min. of arc (Dodge, 1907), but unfortunately all head movements must be recorded and allowed for by the use of a spherical bead (known as a false eye) which is mounted as close as possible to the real eye. For example a head movement as small as 0.075 mm. could be confused with an eye movement of 1 degree (Ditchburn and Ginsborg, 1953). Furthermore the correction procedure breaks down if the head movements are greater than a few millimetres for it presumes that the false eye and the real eye move in the same way as a result of a head movement, which may not be true; the head might rotate about the false eye which would then not reveal the movement.

It is now possible to show why the "Dodge" technique is unsuitable as a basis for perimetry with animals.

- (1) The head movements must be minute if they are to be negligible. The animal would have to be severely restrained.
- (2) A monkey would have to tolerate a small false eye close to one of its own eyes. If detachable the animal should not be able to do this himself. It would need to be replaced accurately.
- (3) As Dodge himself knew the eye does not rotate only about a point in space that is fixed with regard to the head. The eyeball may move backwards, forwards, and sideways whilst the head is quite still. A false eye would not reveal such translatory movements which are superimposed upon pure rotatory movements and yield artefacts in the record of the direction of gaze. The magnitude of the artefact is disputed but it may be several degrees (Tani *et al.*, 1956; Alpern, 1962). What is undisputed is that it has not been investigated in the monkey.

Wendt and Dodge (1938, p. 28) write "Its use (the corneal reflection method) with animals has, so far as we know, never been attempted but there seems to be no

RECOVERY FROM EARLY BLINDNESS A CASE STUDY

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The problem of how this case, and the earlier cases reviewed by Dr. von Senden, can contribute to our knowledge of perceptual learning is discussed.

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reason why it should not be applicable, at least to Primates." In the light of the above three points their statement seems too optimistic unless it refers only to acute studies with restrained animals.

The second common method of studying eye position exploits the potential difference between the cornea and the retina (Mowrer, Ruch and Miller, 1936). If electrodes are suitably placed near the eyes the change in potential is closely proportional to the sine of the angle of deviation of the eyes (Fenn and Hursh, 1937; Miles 1939a). The method is inappropriate for use with monkeys for the following reasons:—

- (1) Although it is not as susceptible to head movements as the corneal reflection technique (Hoffman, Wellman and Carmichael, 1939), such movements must still be restricted. An animal could alter its fixation point by moving only its head and there would be no change in the electrical record from the eye.
- (2) An initial calibration is needed because the resting potential varies widely between different subjects (Fenn and Hursh, 1937; Miles, 1939a, b) and a given change in potential does not necessarily indicate the same eye rotation in different individuals. Moreover equal angular rotations of the eye in different directions in the same subject yield different potentials (Miles, 1939c) probably because of the difficulty of placing electrodes symmetrically about the eye. For these reasons the voltage must first be measured for several angles of regard which must be determined in some other way with an animal.
- (3) At present the stability of the standing potential over long periods of time has not been studied therefore frequent recalibration would be required. According to Fenn and Hursh (1937) and Halstead (1938) the standing potential is constant for one person but neither of them took readings for more than a few days. Miles (1939b) found the potential to be consistently lower when he tested his subjects for the second time.
- (4) When there is a shift in fixation along a horizontal axis the electro-oculograph often indicates a vertical rotation although none exists (as can be shown by other methods and the subject's own report). This artefact may be as much as 10 degrees for a 15-degree sweep to the side (Alpern, 1962). A likely explanation is that there is a vertical shift of the whole orbit which would bring the electrically positive cornea closer to one of the two electrodes recording vertical displacements, but this would not help us to decide whether a particular record was genuine when we were studying an animal.
- (5) The method assumes a stable retino-corneal potential when the eye is not moving. The assumption is questionable. Kohlrausch (1931) reported that the standing potential changes when the illumination to the eye is altered. Miles (1940) found a gradual change of 0.38 mv. when the illumination was increased from 0.001 millilamberts to 5 millilamberts, and Kris (1958) showed that the same eye movements may yield potentials as different as 0.5 mv. according to whether the eyes are dark or light adapted. This difference equals the total change in potential which may be recorded for an eye movement of, for instance, 20 degrees. The method may therefore be unreliable when used with an animal which is free to move and therefore to vary the light reaching its eyes.

An even larger artefact is caused by the galvanic skin response (Miles, 1939a; Jung, 1939) and although this rapidly disappears in human subjects as they become

accustomed to the testing procedure it would be surprising if untamed animals adapted as quickly.

In view of these drawbacks the remark of Hoffman *et al.* (1939, p. 52) that "the electrical technique of eye movement recording can be used with animals" is misleading without a statement of the limitations of the technique.

The third method of recording eye position uses some device attached to the eye itself. The eye may be open or closed, in which case it is assumed to behave in a similar manner to the viewing eye. The eye movements are assessed by recording, photographically or mechanically, the excursions of the attachment. Reviews of the method are given by Grünberg (1924), Vernon (1928) and Wendt and Dodge (1938). Wendt and Dodge succeeded in recording the eye movements of several species of monkey. They used two methods. Firstly they used a mirror recorder which was pressed against the closed eye. Head movements had to be eliminated and the animal was so restrained that little but the eyes could move voluntarily. In the second experiment slight head movements were permissible but a silk thread had to be sewn to the sclera of the eye from which measurements were taken. They were unable to record for more than 3 days because of irritation to the eye.

The third general method of recording eye position is unsuitable for the following reasons:—

- (1) If the movements of a small attachment to the eye are photographed the same inadequacies occur as with the corneal reflection technique.
- (2) In addition an attachment such as a contact lens may slip, especially when the eye rotates. Alpern (1962) says this rotation may be as little as 1 degree before a lens slips and with much larger eye movement it is quite unjustified to assume that no slipping occurs. A suction corneal lens has been developed by Yarbus (1957) and Barlow (1963) and although the slipping is negligible the irritation is not, and the cornea must be anaesthetized.
- (3) If the movements are recorded mechanically the animal must be rigidly harnessed. Irritation and infection of the eye are likely, and re-assembly of the instruments on the eye might be difficult and tedious.
- (4) If the position of one eye is inferred from that of the other eye which bears attachments, it must be assumed that both eyes are fixating the same point. This assumption is supported by evidence from human subjects where neither eye has impediments (e.g. Lord, 1951). However there are doubts that conjugate fixation is always maintained. Stromberg (1942), using the corneal reflection technique, found that the eyes of a human subject who is reading may be fixating points as far as 1 in. apart. If the material is 14 in. from his eyes this represents a fixation disparity of about 4 degrees.

More recent investigators (Tani *et al.*, 1956; Rashbass and Westheimer, 1960) have claimed that this fixation disparity is an artefact of the corneal reflection technique and attribute it to the factor mentioned previously, namely that the centre of rotation of the eye is not a fixed point but lies along a curve. It shifts to the right when the eye rotates to the left, and vice versa. They point out that subjective methods of recording eye position (using after-images or the subject's perception of his own macula in plane polarized light) show fixation disparity to be of the order of minutes of arc, and a recent study employing an objective technique which records rotation independently of lateral shifts (Rashbass and Westheimer, 1961) reaches the same conclusion. Previous objective methods may therefore be unreliable.

However, Verhoeff (1959) disputes the validity of the subjective methods of recording eye position chiefly because the objective methods give results which are

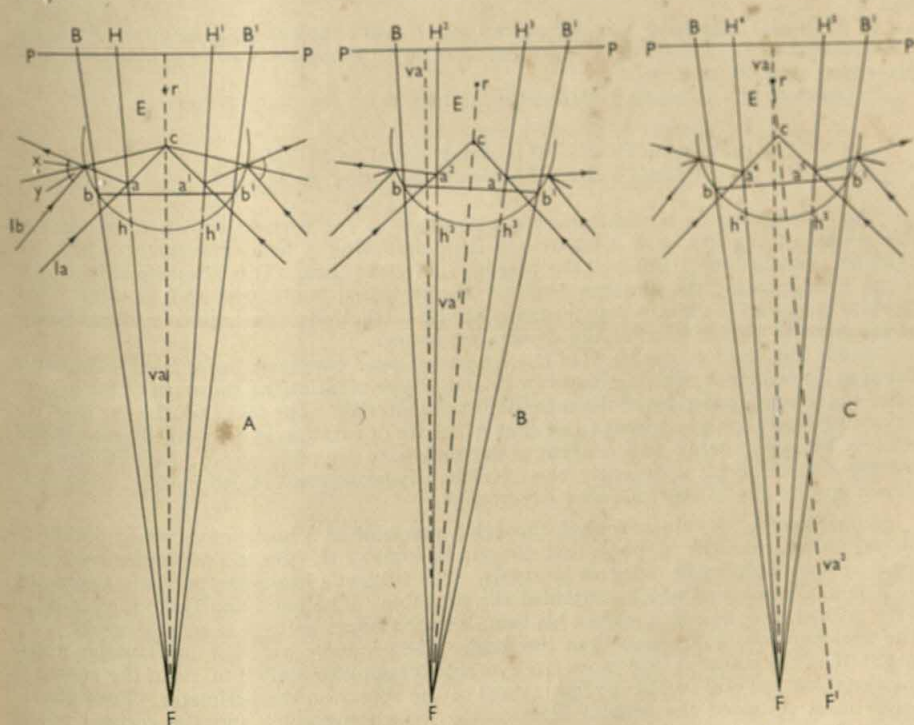
consistent with each other. Other workers (Alpern, 1957; Ellerbrock, 1954) using the subjective methods find a fixation disparity as great as that revealed by objective tests. The issue is clearly unresolved. Moreover the crucial experiment of showing that the eyes share a common fixation point when one of them bears attachments has not been done.

It is concluded that conventional methods of recording fixation are at present inconvenient and possibly inaccurate if used with animals. The problem therefore was to design a new method of recording an animal's fixation that would allow the animal the freedom and composure to be able to make a visual discrimination and to respond appropriately for several months.

METHOD AND RESULTS

The following method was devised and tested with human subjects. It depends upon the fact that although a corneal reflection moves sufficiently far, when the head moves, to

FIGURE 1



The position of two "corneal" reflections on a hypothetical spherical eye when the eye is in various positions with respect to a camera lens F .

spoil a photographic record of the *absolute* movement of the reflection, its movement relative to the eye itself is much less marked provided that the source of the reflection is reasonably far from the eye. The position of the reflection relative to the outer and inner borders of the iris and to the centre of the pupil remains virtually unchanged if the eye maintains its fixation but the head moves, for instance, $\frac{1}{2}$ in.

Figure 1 illustrates this point. In diagram A, E represents a hypothetical eye with its centre of rotation at r and the centre of curvature of the cornea at c . (The relative proportions are those of a human eye and are taken from Ditchburn and Ginsborg, 1953.

They also agree very closely with the mean of the estimates of these two values which have been made in the past—see Duke-Elder, 1942, v. 1, pp. 41 and 591; Alder, 1959, p. 20.) F is a fixation point and is also the centre of a camera lens; b and b^1 are the outer borders of the iris (they could equally well be the inner borders); va is the visual axis; L and L^1 are two sources of light situated at the same distance from E as F is from E but at 45 degrees to the right and left. They are too far from E to be shown in the diagram.

Regarding the cornea as a convex spherical mirror the apparent image of L can be found as follows: la and lb are light rays from L ; la is at right angles to the cornea and is reflected back along its path; lb strikes the cornea obliquely and is similarly reflected so that angle x equals angle y . If the reflected rays are extrapolated behind the mirror they meet at a , which is the position of the apparent image of L . The actual highlight on the cornea, as seen from F , lies at h where the straight line from a to F intersects the cornea. Similarly h^1 is the highlight from L^1 .

Were E to be photographed from F and the resulting negative projected through the same lens system on to the plane surface P the positions of the highlights and the borders would be found at B , H , H^1 , and B^1 .

In diagram B, E has moved considerably to the right of F as a result of a "head movement," but has refixedated F ; va^1 is the new visual axis. The highlights from L and L^1 now lie at h^2 and h^3 . On projection the highlights and borders are now at B , H^2 , H^3 , and B^1 .

In diagram C the head has not moved but E has changed its fixation to F^1 , about 5 degrees from F . The corneal reflections now lie at h^4 and h^5 , still seen from F . After projection they lie at H^4 and H^5 .

Considering the distances between the landmarks on the plane surface P ;

In diagram A: $B-H$ is equal to B^1-H^1 .

In diagram B: $B-H^2$ is almost equal to B^1-H^3 .

In diagram C: $B-H^4$ is clearly not equal to B^1-H^5 .

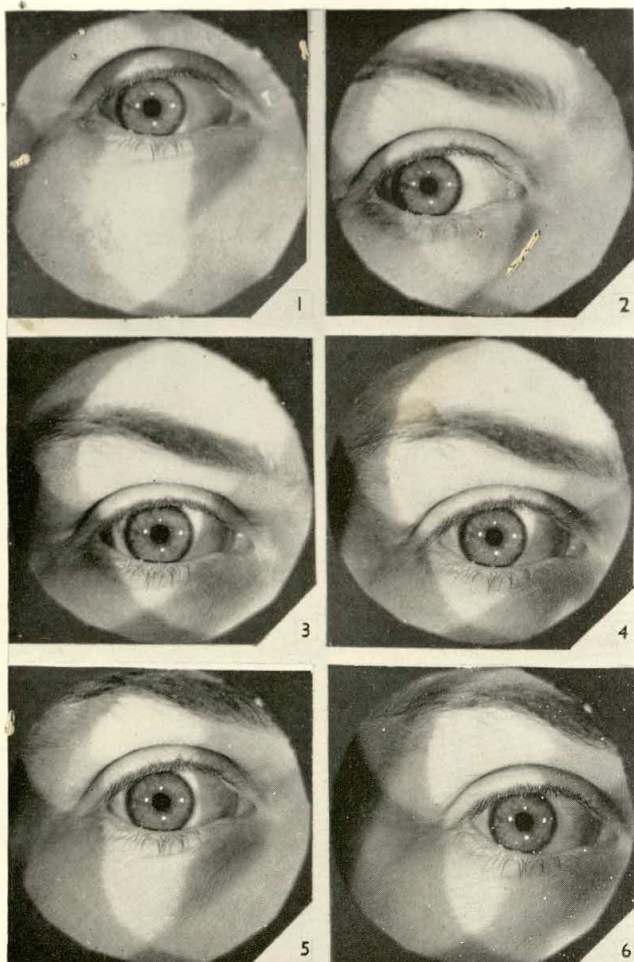
Thus when fixation is maintained the positions of the corneal reflections relative to the border of the iris have altered very little following a head movement which has changed the absolute position of the reflections a great deal. This absolute shift can be found by measuring the distance between the projected landmarks and va . But when fixation changes by about 5 degrees the positions of the reflections relative to the border of the iris also change in a readily detectable manner.

If only the absolute positions of the reflections were considered confusion would arise between a head movement, as shown in B, and an eye rotation, as shown in C, for in both cases the absolute positions of the reflections have altered. The fact that the eye does not have a perfectly spherical cornea and that its centre of rotation is not a fixed point means that its behaviour only resembles that of the imaginary eye of Figure 1. Nevertheless the correspondence seems sufficiently close for the dissimilarities not to matter. This was shown empirically in the following experiment.

Experiment 1. A subject looked through a peephole at a horizontal curved perimeter placed 50 cm. from the peephole and directly in front of it. On the perimeter were nine fixation points placed at 5-degree intervals. The subject's head was partially restrained by a box, the front of which contained the peephole. The back and bottom of the box were open so that he could retract his head between fixations, which he was asked to do. The enclosure was 3 in. wider than the head. The peephole was 2 in. in diameter which meant that the subject could move his eyes up to approximately $\frac{1}{2}$ in. from the centre of the peephole and still be able to fixate most of the points on the perimeter. Four photo-flood bulbs provided the general illumination for photography and the corneal reflections. Two were placed 45 degrees to the right and left of the eye, and two above and below it in the sagittal plane. Each was 100 cm. from the eye. The camera lens was placed as close to the central fixation point as possible, i.e. almost directly in front of the peephole.

The subject fixated each point on the display 30 times and his viewing eye was photographed on each occasion. The photographs were compared to see if all those for any one fixation point were identical, or very nearly so, despite head movements. This was done by enlarging the photographs by 20 diameters and examining them. It was found that all photographs for any single fixation point were essentially similar although the head had moved sideways up to $\frac{1}{2}$ in.

FIGURE 2



Photographs of the right eye of a subject who was fixating the same point on each occasion. Head movement with respect to the peephole was allowed.

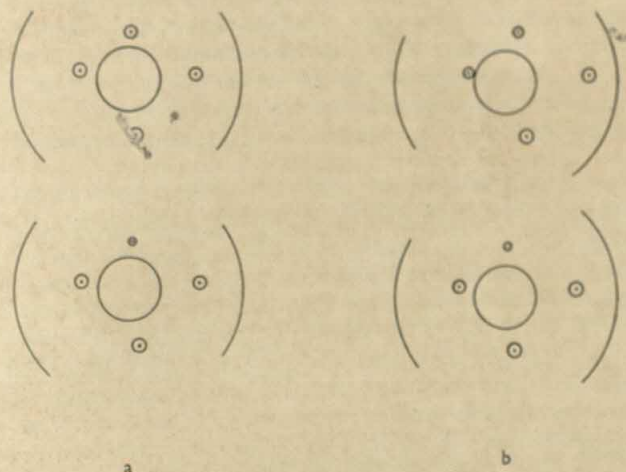
FIGURE 4



A record of the subject's eye when fixating adjacent points on the display.

Figure 2 shows a series of six photographs taken whilst the same point was being fixated. The head movements are up to $\frac{1}{2}$ in. and in each photograph the eye occupies a different position relative to the peephole. The positions of the four highlights relative to the contours of the eye are the same in all the photographs. This is seen more easily in Figure 3a where two photographs of the eye when it was looking at the same fixation point have been enlarged, the relevant details on the eye drawn, and the drawings photographed. The pupils differed in size in the two photographs

FIGURE 3



Diagrams of the eye shown in (a) photographs 2 and 6 of Figure 2 and (b) photographs 6 and 5 of Figure 4. For explanation see text.

and were made equal by enlarging one of them slightly. The head had moved $\frac{1}{2}$ in. to one side between taking the two photographs.

Figure 2 should be compared with Figure 4 which is a record of the subject fixating adjacent points on the display. The orientation of the highlights is different in all the photographs. Figure 3b demonstrates this more clearly. Photographs of the eye fixating points 5 degrees apart have been enlarged and drawn. This Figure should be compared with Figure 3a.

Two further experiments were performed to see if a subject's locus of fixation could be successfully determined using the method suggested.

Experiment 2. Two subjects took part. The subject looked through a peephole at a curved white surface bearing 99 fixation points, small black "x's" each subtending about 15 min. at the eye. Each point was numbered and lay 5 degrees from neighbouring points 15 min. above, below, and to the sides. The points were arranged in nine horizontal rows, 11 to each row. The peephole was the same as that already described but the head restriction was more severe so that only with difficulty could the subject move his head more than $\frac{1}{2}$ in. from a position where the eye was centrally placed at the peephole. An 8 mm. cine camera, set to take single exposures, was mounted so that the lens was directly in front of the peephole and 50 cm. from it. The base of the screen lay just above the lens and this arrangement ensured that the subject never had to look down at the display, for when this occurs the upper lid inevitably obscures the pupil and the corneal reflections.

The subject fixated each point in turn in the manner in which he would read a page of print, starting at the top left. A single photograph was taken of each fixation. These constituted the standard reference photographs, which henceforth will be called the

"standards." The subject was now asked to fixate any of the points on the screen; each fixation was photographed and these formed the "unknowns." Following each fixation he withdrew his head from the restrictor and recorded the identity number of the point he had fixated. More than 200 "unknowns" were obtained from each subject and the first 200 in which the photograph was not marred by blinks or movements were analysed in the following way. The "standards" were enlarged by a factor of 50 diameters and made into positive prints. Each "unknown" was examined with hand lens ($\times 15$ diameters) and the appropriate matching "standard" selected from the series. The "unknowns" were analysed twice by this method and any discrepancies between the two sets of determinations sought. These particular "unknowns" were re-examined.

Results. For one subject the discrepancy between the two sets of evaluations amounted to 4 per cent., i.e. on eight occasions a certain "unknown" was judged differently on second viewing. They were re-examined until the experimenter was satisfied. There was no discrepancy for the second subject. The final set of determinations of the locus of fixation was compared with each subject's own record and in both cases the coincidence was complete. All fixations were correctly identified.

Experiment 3. This experiment was performed with a single subject and using the apparatus which was built as the perimeter for use with monkeys. The subject looked through a peephole 1 in. in diameter. Three pieces of metal, two of which were fixed by the sides of the peephole and the other above it, were adjusted so that he could move his head only about $\frac{1}{4}$ in. whilst looking through the peephole with his right eye. Fifty cm. from his eye and at the same height from the ground was the camera lens. Above this was a curved perimeter whose surface was part of a sphere with the concave side towards the eye, and the centre of curvature at the peephole. Small bulbs were mounted in the screen so that each was 5 degrees from its neighbours above, below, and to the sides. Fifty-six of these bulbs were selected as fixation points. They formed a grid consisting of eight rows with seven bulbs in each row, and covered an area in the visual field of 30 degrees by 35 degrees. An identity number was placed next to each bulb. The four light sources which provided the general illumination and the corneal reflections were mounted so that the first was 37 degrees above the centre of the perimeter, the second 50 degrees below it, and the remaining two 45 degrees to either side. The lowermost of the four large bulbs was 25 cm. from the peephole; the others were 35 cm. away. Each subtended about 6 degrees at the subject's eye.

The "standards" were obtained first. The subject then made more than 500 fixations as he pleased. After each one he withdrew his head and wrote down the identity number of the bulb which he had fixated.

The "standards" were made into enlargement prints as before and mounted on to a large board. Each "unknown" was projected upon a plane white surface alongside the "standards" and the matching was done by scanning the array of "standards" until the appropriate one was found. This was a more rapid procedure than that used in Experiment 2. Only when the subject had fixated a bulb in the upper corners of the display was matching at all difficult for one or two of the highlights disappeared from the edge of the cornea. Whenever this occurred a pair of calipers was used to measure accurately the distance of the remaining highlights from the outer and inner borders of the iris.

Results. The first 500 clear photographs were used and 497 of these were correctly identified. The remaining three were incorrectly judged by 5 degrees.

The pattern of the subject's fixations revealed that although he did not look at each bulb equally often he did sample all the available positions.

It is concluded from Experiments 2 and 3 that this method affords a reliable and valid estimate of the locus of fixation to the nearest 5 degrees and that it is a suitable one to use with monkeys because small head movements do not upset it.

DISCUSSION

The orientation of the reflections on the eye was considered with respect to the inner and outer borders of the iris. Monkeys have no "white" to the eye and no clear outer iris border exists. This means that only the inner border can be used

with these animals. However, the position of the inner border depends on the size of the pupil which occasionally varies, even with a human subject who looks at the same point in the visual field on successive trials. Fortunately, when the pupil changes in size its border appears to move toward or away from each corneal reflection by an approximately equal amount (exactly equal when the eye is looking at the camera, very nearly equal when it is looking elsewhere and the pupil appears slightly elliptical). Pupillary variation can accordingly be allowed for.

In considering the reliability of the proposed new method a study by Jones, Milton, and Fitts (1949) is relevant. They wished to know how pilots looked at instrument dials during flight and the conditions under which the pilots were studied had to be as natural as possible, a criterion which also had to be met here. They mounted a cine camera behind the pilot and photographed the reflection of his eyes in a mirror, having first obtained standard reference photographs. They determined his locus of fixation from the position of his eyes and head. More than 40,000 frames of film were analysed separately by different observers and more than 90 per cent. were read identically. Most instances of disagreement involved photographs which "were not as clear as would have been desirable" (p. 21). The authors were concerned with only eight of the 22 dials in the instrument panel but their report does not reveal which of them they used. It is therefore difficult to tell how small an angular difference they could reliably detect. Taking into consideration the shape and dimensions of the cockpit and dashboard it would appear that the angular distance between adjacent dials varied from 3 to 12 degrees at the pilot's eyes.

Unfortunately their study does not distinguish between reliability and validity. They imply that when independent observers interpret the film identically they are interpreting it correctly, which may not be true. They did not report any experiment in which their determination of the pilot's fixation was checked against the pilot's own record of them.

However, perhaps the most important point in relation to this paper is that the assessments of fixation were made from the position of the pilot's eyes and head without regard to the position of any reflections on the eyes. Their use, as described here, should greatly increase the reliability of the technique.

A last point concerns the precision of the technique. If fixation is correctly referred to the nearest point in a grid of points which are arranged 5 degrees apart and in vertical and horizontal rows the maximum error involved will be half the angular distance between two points on a diagonal, i.e. close to $3\frac{1}{2}$ degrees. This is too coarse a measurement to be used in the investigation of certain visual problems, such as macular sparing. But it is sufficiently fine for the study of the size, shape, severity, and position of visual field defects which are more than 5 degrees across, for which it has been used with rhesus monkeys (Cowey, 1961).

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A PERIMETRIC STUDY OF VISUAL FIELD DEFECTS IN MONKEYS

BY

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The visual fields of rhesus monkeys have been studied perimetrically before and after removal of parts of the striate cortex. The operations produced visual field defects of the expected size, shape, and position but an animal's ability to respond to a flash of light which appeared in the defective part of the field was diminished rather than abolished. It is suggested that this residual ability enables an animal to detect changes in illumination, which might be cues to other visual events. A study of fixation indicates that the animals probably do not recognize or respond to objects when they lie within the impaired region of the visual field. The results are compared with those found in earlier studies of simian and human subjects.

INTRODUCTION

The study of visual field defects has helped us to understand the human visual system. Injury to part of the striate cortex is usually said to produce a clearly defined region of *total* blindness in the visual field and this is strong evidence that in man there is a point-to-point projection of the retina to the striate cortex, and that vision is completely dependent on the integrity of this projection pathway.

Anatomically the visual systems of the rhesus monkey and man are very similar. One therefore expects damage to the striate cortex to have similar effects on vision in the two species. The demonstration that this is true has long been sought and never satisfactorily realized, but it is not clear whether the methods used to display field defects in animals were inadequate or whether the defects did not exist in as severe or permanent a form as expected. For example Munk (1881) reported permanent scotomata in dogs after unilateral occipital lobectomy. His animals failed to respond to objects presented on the abnormal side of the visual field. The symptom rapidly disappeared in monkeys but Munk believed that the animal swiftly compensated for a hemianopia by a change in fixation. Loeb (1884, 1901) observed similar symptoms but concluded that they indicated amblyopia and not scotomata for his animals invariably responded to a moving stimulus although not to a stationary one. Hitzig (1903) was impressed by the evanescence of the symptoms which followed damage to the occipital lobes in dogs and he, too, interpreted the defect as an amblyopia. Luciani (1884) carried out similar operations on dogs and monkeys and although they could find objects by sight they were unable to distinguish between food and inedible objects on the basis of vision alone. He described the defect as psychic blindness which is close to the view expressed by Goltz (1888) that the impairment was intellectual rather than sensory.

Minkowski (1911) found that he had to destroy at least half of the visual cortex on one side in dogs before any demonstrable deficit followed. He believed the defect to be a scotoma but it is not clear whether his animals overlooked food because they had not seen it or because they had not recognized it. Ferrier (1886) noticed only a transient visual defect after removing both occipital lobes in the monkey and Horsley and Schäfer (1888) reported almost complete recovery of vision after the same operation. Franz (1911) used a variety of learned discriminations as a measure of

visual ability after bilateral occipital lobectomy and observed that the animals recovered until they appeared normal. Occipital lobectomy does not remove striate cortex from the anterior part of the calcarine fissure but it should be sufficient to produce a large central field defect.

Other workers have been more definite about the existence of field defects. Dusser de Barenne (1933, p. 899) states that . . . "Recovery from visual disturbances after lesions of the area striata is observed only to a limited extent; quadrant hemianopia and total homonymous hemianopia are permanent symptoms even in the lower animals" and he asserts that it is adjustment of fixation which makes recovery apparent. Harlow (1939) and Settlage (1939) incline to the latter view. They showed quite clearly that after unilateral occipital lobectomy the rhesus monkey fails to use visual cues on one side of the old field of vision. Recovery of the ability to solve crossed-string puzzles and to respond to startle stimuli on the blind side was said to coincide with a change in bodily orientation and visual fixation. However, they admit that this change later became undetectable (and therefore non-existent?) although the final visual capacity of the animals was as good as before operation.

Settlage also removed areas of various size from the remaining occipital lobe of his monkeys and found no performance deficit of any kind in tests of brightness discrimination, pattern discrimination, or visual searching ability. Despite the extraordinary nature of this finding he considered that . . . "permanent visual sensory deprivation did occur as a result of the lesion" (p. 109), and assumed that the animals were extremely adept at scanning the visual field with a remnant of the visual system. Nevertheless he did recognize the inconclusiveness of his interpretation because "There is no known method of perimetric examination of the monkey's visual field" (p. 106).

Even total removal of the striate cortex in monkeys leaves some sensitivity to light. Klüver (1936, 1937, 1941, 1942) in a series of extensive studies concluded that the destriated monkey could react to light and discriminate differences in total luminous flux but that otherwise he was blind and remained so. Denny-Brown and Chambers (1955) found the results of this operation less severe. Their monkeys could still perform visual placing responses and grasped at moving objects. Pattern vision was apparently absent but Weiskrantz (1963) has shown that a rhesus monkey, after the same operation, can discriminate between patterns which differ in total length of contour. These studies tell us a great deal about the animal's general visual capacity but they do not reveal whether there was any region of total blindness or whether the defect was a severe amblyopia involving the entire visual field.

The few experiments we have mentioned are representative of a large number carried out in the past 100 years. The conclusion is inescapable that there has been no convincing demonstration of a permanent scotoma in a single animal with the possible exception of the frog, which rarely moves its eye with respect to its body, thus permitting measurements of field defects (Sperry, 1944). The present study attempts to improve our observations on the monkey by studying field defects perimetrically. An effort is made to define the characteristics of the defects precisely, and to relate the findings to our ideas about the organization of vision. A preliminary note has appeared elsewhere (Cowey, 1962).

1. METHOD

The perimetric procedure used here is based upon a method of recording an animal's locus of fixation to the nearest 5 degrees at the instant a visual stimulus is presented. The viewing eye is photographed and the photograph is later compared with a set of standard reference photographs. The latter consists of a record of the animal fixating up to 64

known points in the perimeter spaced at 5-degree intervals. The technique has been described in detail elsewhere (Cowey, 1963).

Apparatus

A diagram of the testing apparatus is shown in Figure 1. The perimeter was mounted in a testing enclosure measuring 6 ft. long by 5 ft. high by 3 ft. wide. The animal's cage was fastened inside one end of the enclosure. The door of the cage was removed by sliding it out and the cage was pressed against the face of the apparatus shown in Figure 1. The animal thus faced a peep-hole, 1 in. in diameter, to which he could put his right eye

FIGURE 1

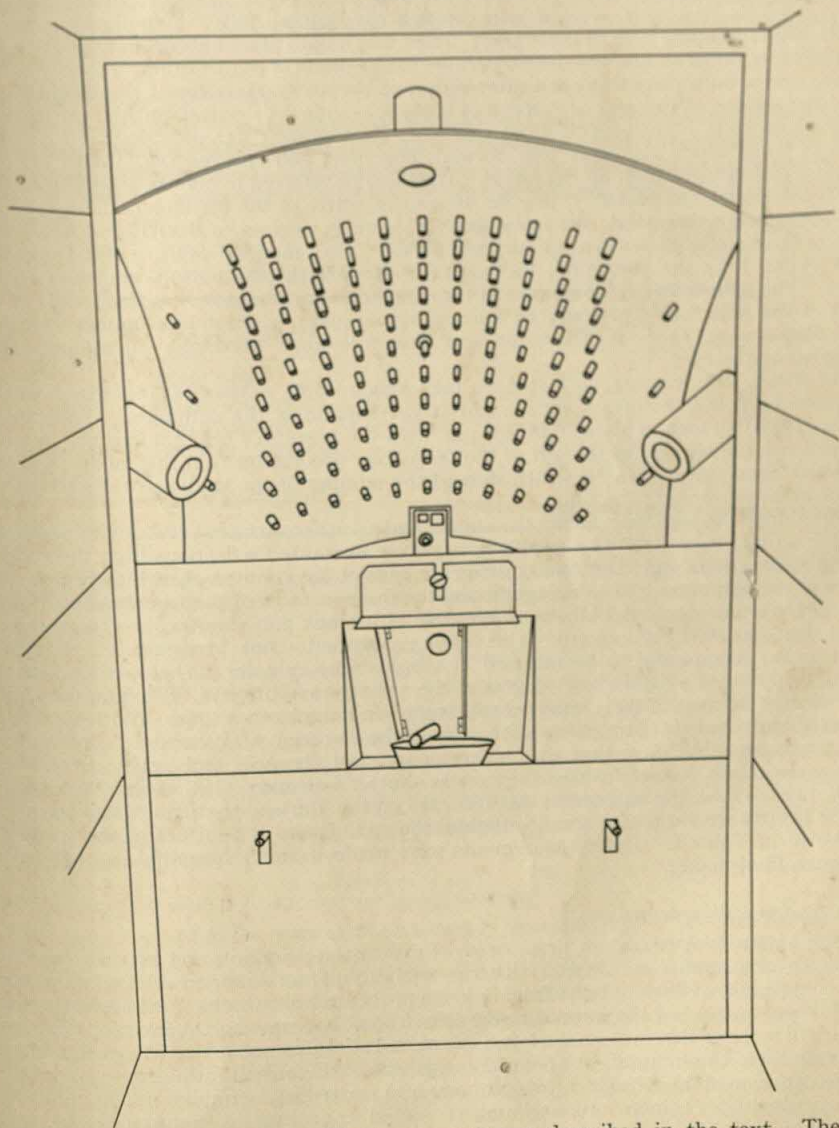


Diagram of the perimeter. The components are described in the text. The wall above the peep-hole is omitted to reveal what lies behind. The animal's cage is not shown.

by forcing his head between two thin vertical strips of metal each 6 in. long by 3 in. wide. A similar piece of metal was placed horizontally above the peep-hole to restrict vertical head movements. On a small recess below the peep-hole was a reward bowl, into which food pellets could be automatically delivered from a concealed feeder. Seven in. below the peep-hole were two levers, one 6 in. to the right of the peep-hole, the other 6 in. to the left. They could be pressed down by the animal.

The perimeter bowl lay 50 cm. from the peep-hole. It consisted of a curved screen, part of a sphere of radius 50 cm. and was mounted so that its lower edge was level with the peep-hole. Any point on its concave side was 50 cm. from the peep-hole. Set into the perimeter were 121 small stimulus bulbs (Mazda, X95 1271, 28 v., 3.5 w.) at 5-degree intervals. A small white circle was painted around the central bulb. The precise configuration of the bulbs was as follows: Two great circles, one in the vertical plane, the other at 90 degrees to it, were drawn through the central bulb. Five-degree intervals were marked off along both of these great circles, and a bulb placed at each mark. Through each of these bulbs an arc was drawn parallel to the plane of one or other of the two great circles, and a bulb placed at each intersection of the arcs. As a result the bulbs lay on the intersections of two sets of "circles of latitude"—one set vertical and the other set at right angles to it. The general illumination for photography was provided by four large 60-watt bulbs which also gave four corneal reflections and ensured that there was considerable background light on the retina. They will be referred to here as floodlights. One of the floodlights was placed 37 degrees above the centre of the perimeter, the second lay 50 degrees below this point; the remaining two were 45 degrees to the left and right of it. Each of the floodlights was in a cylindrical reflector. The "mouths" of three of the reflectors were 35 cm. from the peep-hole; the mouth of the fourth (the bottom one) was 10 cm. closer. The light from each bulb passed through a circular blue filter (Actina, $\times 1\frac{1}{2}$) at the front of the reflector. This reduced uncomfortable glare, at least to a human observer. The filters were $1\frac{1}{2}$ in. in diameter and therefore each floodlight subtended about 6 degrees at the eye.

An 8 mm. cine camera, adjusted to take single-frame exposures and fitted with a telephoto lens, was set into a square gap at the bottom of the perimeter. The camera photographed an area of about 6 in. by 4 in. with the peep-hole near the centre. A small plane mirror, above and to the left of the peep-hole, was included in this field. The face of an electromagnet counter, which showed the number of the trial, was reflected in the mirror and appeared on the film.

The side of the perimeter facing the animal and all other surfaces between the perimeter and the animal were painted matt-black to reduce unwanted reflections from the stimuli. The only exceptions were the small mirror, the face of the counter, the white circle at the centre of the perimeter, the camera lens, and the front surfaces of the stimulus bulbs. The sides of the small stimulus bulbs were covered with black polyvinyl tape so that all light from them was directed forward. The coating extended about $\frac{3}{8}$ in. past the end of the bulb so that it appeared to be recessed in a light-tight cylinder. The open end of this cylinder subtended a little over 1 degree at the animal's viewing eye. The stimulus bulbs were selected because of their rapid incandescence and nigrescence times. With a $1/20$ sec. pulse they reached full brightness within the pulse duration. Pulses were synchronized with the camera shutter so that with a shutter speed of $1/17$ sec., and a pulse of $1/20$ sec., the complete flash occurred whilst the camera shutter was open. If the animal moved his eye during the flash the movement was recorded on the film and that trial was discounted.

The luminance values for the floodlights, the stimuli and the surface of the perimeter are shown in Table I. The measurements were made from the peep-hole with a S.E.I. Exposure Photometer.

Training and testing procedures

Each animal was taught to press its right eye to the peep-hole and await a stimulus. Two kinds of stimulus were used—either a brief noise from a buzzer, or the same noise plus a simultaneous flash of light from any one of the stimulus bulbs. Whenever the first stimulus was presented the animal could obtain food by pressing the lever on his right. He learned to press the lever on his left if the paired stimulus was presented. If he pressed the wrong lever the house light above the cage was extinguished for half a second.

The experimenter sat behind the perimeter and started each trial by pressing a button. This operated the camera shutter and presented the stimulus simultaneously. The experimenter could see the animal's eye and attempted to present a stimulus only when the animal was looking towards the centre of the perimeter. It was hoped that the animal

would learn to look at the white circle around the central stimulus and that this would simplify the analysis of the eye position. Up to 200 trials were usually given in one session.

The two kinds of stimuli were presented equally often and in a pre-determined but random order by means of a uniselector with 63 positions. A different random arrangement was used each week and a different starting point in the series was chosen each day. The animal's performance was recorded automatically on a series of counters and a 12-channel paper recorder. The following events were recorded; total number of trials, the identification number of the particular stimulus bulb used on any trial in which the paired stimulus was presented, number of correct and incorrect responses and the position at which they occurred in the sequence of trials, failure to respond (which was rare). It was therefore straightforward to determine whenever an animal had responded incorrectly to a visual stimulus. The eye photograph for that trial was examined in order to find the animal's fixation point.

Standard reference photographs

These were obtained as follows. Each animal was trained for several months in the perimetric apparatus during which time nothing unusual was presented to its view. Then a tiny unshelled peanut was pinned to the front of one of the stimulus bulbs for 15 consecutive trials. The experimenter could see that the animal was looking at the peanut

TABLE I
LUMINANCE VALUES IN FOOT LAMBERTS

Source	Luminance
Flood light	5,000
Bright stimulus	1,125
Dim stimulus	11.25
Black background	0.10 at centre 0.25 at edge
White background	1.75 at centre 2.50 at edge

and this impression was confirmed when about 10 or more of the 15 eye photographs proved to be identical. This procedure was repeated for up to 64 positions in the perimeter embracing 35×35 degrees of the visual field and it was carried out with each animal. There is one objective piece of evidence which supports the fact that this method of obtaining the reference photographs was valid. The four floodlights which provided the highlights on the eye were so grouped about the peep-hole that if the centre of the perimeter highlights on the eye were so grouped about the peep-hole that if the centre of the perimeter highlights were to lie on the optic axis of the eye a photograph of the type shown diagrammatically in Figure 2 (A) would be obtained. Since the visual axis of the right eye lies about 5 degrees in nasal to the optic axis (Polyak, 1957 pp. 208-10) a photograph of the type shown in Figure 2 (B) would be expected when the centre of the perimeter lay on the visual axis, i.e. when the animal was fixating this point. When the fixation procedure was followed for the stimulus bulb in the centre of the perimeter photographs like that of Figure 2 (B) were obtained with each animal.

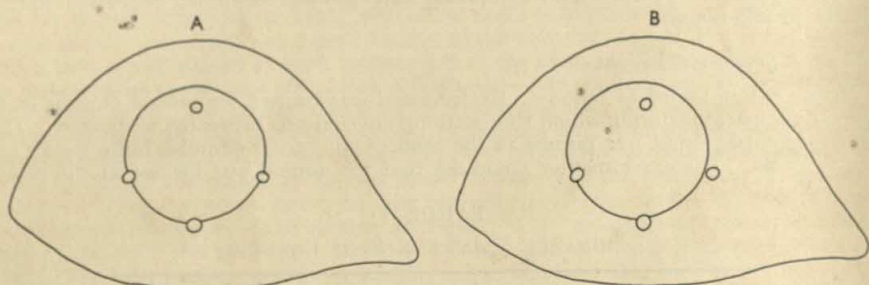
Calculation of the retinal position of the visual stimulus

It is necessary to know more than an animal's fixation point in order to decide where a given stimulus lies in the visual field; the orientation of the vertical retinal meridian must also be known, otherwise, to take the extreme example, an animal might stand on its head and the designated retinal position of the stimulus would be incorrect by 180 degrees. (Actually, this extreme case could not occur because of the shape of the head restrictors.)

One important deviation of the vertical retinal meridian from the true vertical, when the head is held erect, derives from the torsion of the eye when it rotates obliquely with respect to its horizontal and vertical axes. Thus, when the line of fixation of the eye is, say, 45 degrees to the right and 45 degrees upwards, its vertical meridian is no longer parallel to objective vertical. Estimates of the amount of torsion in the human eye have

been presented by several authors (Tschermak-Seysenegg, 1952; Quereau, 1955; Duke-Elder, 1942, Vol. 1, Chap. 13). For the most extreme oblique fixation position in the present experiments, the predicted degree of torsion would be of the order of only 3 to 4 degrees, and therefore torsion has been ignored in calculations of the retinal position of the visual stimuli. In addition, the animals rarely fixated in an extreme direction—the most common fixation point was the central bulb, for which torsion is zero. Estimates of torsion for the human eye may not apply strictly to the monkey eye, but a rough independent measure of torsion for the monkey was made by measuring the departure from vertical of the imaginary line joining the upper and lower corneal reflections on the standard eye photographs. Such measurements, for extreme oblique fixation positions,

FIGURE 2



Outline diagrams taken from photographs of the right eye of a monkey when it was fixating an object (A) 5 degrees to the left of the centre of the perimeter, and (B) at the centre of the perimeter. The orientation of the highlights agrees with that predicted from a knowledge of the anatomy of the eye.

are of the same order of magnitude—3 or 4 degrees—as estimates of torsion of the human eye.

There is one further way in which the vertical retinal meridian can tilt with respect to vertical. Any rotatory movement of the head about its antero-posterior axis would change the orientation of the retina by a similar amount. The head restrictors prevented the monkey from doing this to any appreciable extent, but slight rotations probably occurred. They are ignored here.

Subjects

Four immature male rhesus monkeys were used. With three of them an attempt was made to remove the lateral occipital area bilaterally by means of sub-pial suction. This region is the lateral part of area OC of von Bonin and Bailey (1947) the macular projection area of Talbot and Marshall (1941) and Daniel and Whitteridge (1961). This part of the striate cortex extends anteriorly to within 2 mm. of the lunate sulcus, posteriorly to the occipital pole, and ventrally to the edge of the inferior occipital sulcus. The left occipital lobe was entirely removed from the fourth animal and the remaining striate cortex in the anterior calcarine fissure was sucked out. The inferotemporal cortex had previously been removed from this animal with no effect upon either its visual acuity (Weiskrantz and Cowey, 1963) or its visual fields (the effects of this operation on the visual field of monkeys will be reported in a separate publication). The ablations were conducted under strict aseptic conditions and under deep anaesthesia (Nembutal: Sodium pentobarbitone).

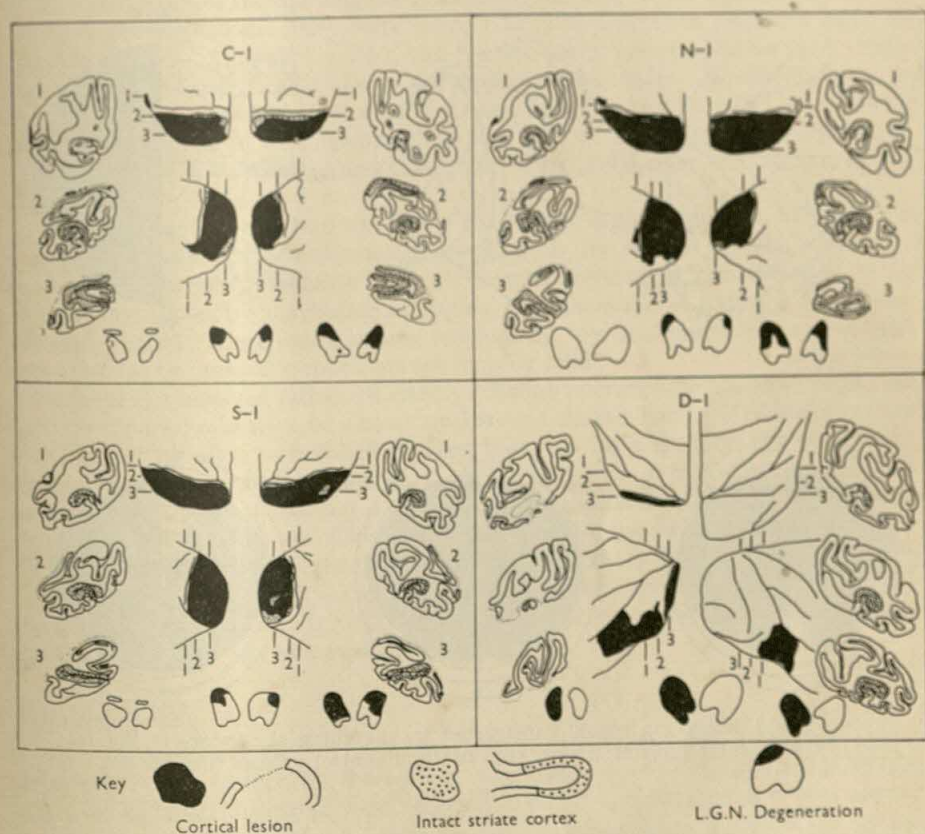
At the end of the experiment the animals were killed with an injection of Nembutal and the brains removed. Each brain was photographed before being fixed in formal-acetic and embedded in paraffin wax after the usual intermediate stages. Sections were cut to 20μ and every twentieth section was stained with cresyl violet. Additional sections were stained according to the Weil method whenever it was necessary to examine the visual afferent fibres entering the striate cortex.

The histological reconstructions are shown in Figure 3. It can be seen that in the first three animals tiny parts of the lateral striate cortex are intact, and in predicting the probable field defects we have considered that they were functional. This conservative

view may not be true, especially for those areas which were surrounded by lesion, for the afferent fibres to these regions were certainly damaged and there was no trace of intact cells within the degenerated portion of the LGN. In the fourth animal a small section of intact striate cortex was found in the ventral limb of the anterior calcarine fissure but its afferent fibre connections were severed and the left LGN was completely degenerated throughout its length, i.e. only glial cells and a few poorly stained and shrunken primary cells remained. A detailed analysis of the geniculo-striate relations revealed by this study will be published in a later paper.

Visual field defects can be predicted for each animal by comparing the form and extent of the lesion with the electro-physiological maps which have been constructed for

FIGURE 3



Histological reconstructions of the brain lesions.

the rhesus monkey (Talbot and Marshall, 1941; Daniel and Whitteridge, 1961). Two views of the electrophysiological projection of the macula are shown at the top of Figure 4. The dorso-posterior view on the left is modified from that of Talbot and Marshall (1941), the posterolateral view is based partly on their text and on the previous Figure. The figures *within* the area striata refer to the angular distance from the fovea. The remaining figures refer to the meridia of the visual field.

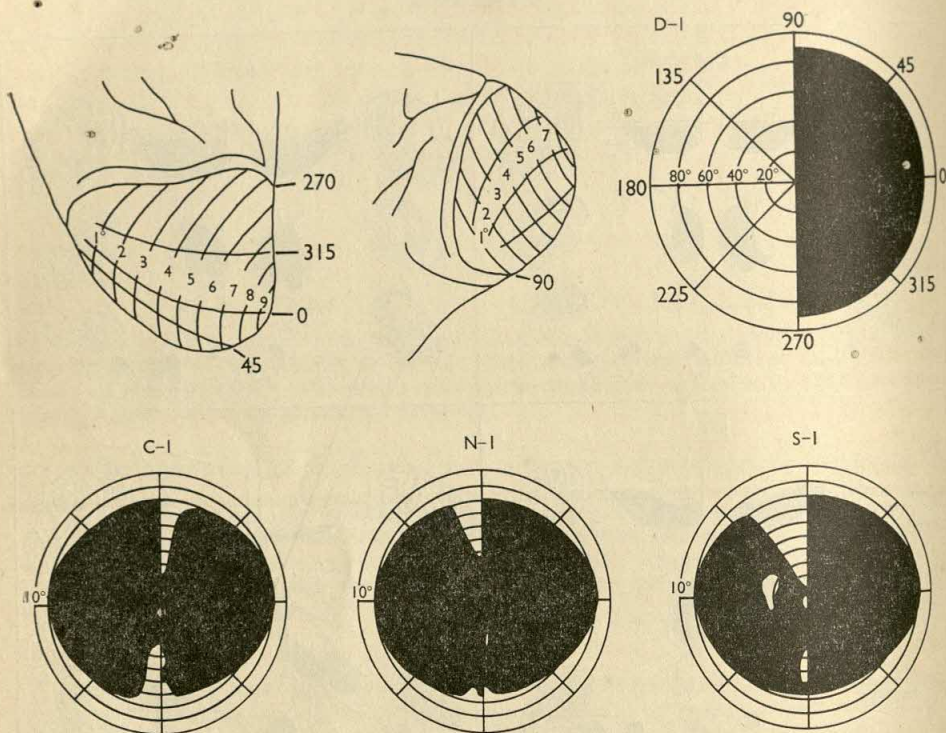
Expected field defects are shown at the right, and below, in Figure 4. The accounts of Talbot and Marshall and Daniel and Whitteridge differ slightly concerning the projection of successive degrees of the macula, and the position of the outer boundary of the defects for the first three animals, especially where it lies about 8 degrees from the fovea, may be correct only to the nearest one or two degrees.

The defect for animal D-1 is presented as a semicircular hemianopia. If the tiny portion of spared striate cortex *were* functional one might expect some sparing along the vertical meridian of the field about 30 or more degrees above the fixation point. This statement is deduced from the account of the projection of the perimacular retina presented by Daniel and Whitteridge and from the unpublished observations of the senior author.

Simple tests for field defects

These were used, in addition to the perimeter. The animal's observation of food, inaccuracies in reaching for it, his hand preferences, and the order in which several pieces

FIGURE 4



Expected field defects (in black) constructed by matching the lesions of Figure 3, with the map of the macular projection area (top left and centre). Note that the scale for D-1 differs from the others.

of food were picked up, were studied as follows. The animal was placed in a small cage with a screen at one side. The screen prevented the animal from seeing food placed on a flat board level with the base of the cage. When the screen was removed the animal saw the food and retrieved it. His performance was photographed and errors were also noted on paper. A grid of rectangular equidistant co-ordinates, 1 in. apart, was present on the otherwise white board and this assisted the scoring of the errors recorded on film.

RESULTS

General observations

After recovering from anaesthesia animals C-1, N-1, and S-1 had difficulty in grasping food accurately when it was handed to them. They also moved their head and eyes a great deal whilst groping for food placed beside them. This behaviour

passed off within 24 hr. and no visual abnormality was ever seen thereafter in their home cages. At first animal D-1 appeared to be hemianopic after the operation. He frequently overlooked food presented on his right and when food was moved from his right towards the centre of the field he did not respond until it reached approximately the mid-line. This behaviour gradually diminished and disappeared within a week. Thereafter the animal seemed quite normal in his home cage or in a play enclosure.

Simple tests

Before operation the animals rarely fumbled for food whilst reaching for it, nor did they overlook food. They were tested 6 hr. after operation and then daily for 10 or more days. Animals C-1, N-1, and S-1 frequently mis-reached for food at first and often moved their head and eyes as though to re-locate it. The effect, which has been described in more detail elsewhere (Covey and Weiskrantz, 1961), was obvious at first, became less so, and disappeared about a week to 10 days after operation. The severity of the symptom and the course of recovery were very similar in the three animals.

There was one further indication of a field defect in these animals. If food was presented on the experimenter's hand for several trials, the animals would subsequently reach and fumble on his *unbaited* hand, all the while moving the head and eyes. This phenomenon did not occur before operation, it could often be detected for several subsequent trials before extinguishing temporarily, and it disappeared at about the same time as the animal stopped misreaching for food.

Animal D-1 behaved rather differently. Before operation he preferred to use the right hand; after operation he almost always used the left hand. He did not fumble for food but he often overlooked it when it lay to his right after the screen was removed. This passed off in a few days. When the pieces of food were placed in line parallel to the front of the cage he selected those on his left first and those on his right last of all or not at all. This occurred even if the line was placed at an angle to the cage so that the food on the left was farthest from the animal who had to struggle to reach it. This behaviour was observed whenever the animal was tested during the 3-month survival period. It was the only indication to a discerning onlooker that the animal's vision was abnormal.

Perimetric testing

(1) *Pre-operative testing.* The performance of each animal was stable, and better than 90 per cent. correct, for 8 weeks or more before operation. The trials for the last 4 weeks before operation were analysed in detail and the overall results appear in the first part of Table II. A "false positive" response is one in which the animal incorrectly presses the left lever, i.e. he reports a non-existent visual stimulus. "False negative" responses occur when the animal fails to report a real visual stimulus, by pressing the lever on his right. These are the responses which ought to reveal field defects. Both kinds of error were often associated with trials on which the film showed the eye to be closed or moving and these trials were discounted. Table II therefore includes only errors which occurred when the animal's eye was open and motionless.

The poorest performer before operation was N-1 with 92.6 per cent. of all trials correct, the best was animal D-1 with 98.6 per cent. correct. This animal had sustained a bilateral inferotemporal removal 6 months before the present operation and had more experience with the perimeter than the other animals. All data which

are here termed pre-operative for this animal refer to his behaviour *after* the earlier operation but *before* the occipital lobectomy.

The "false negative" errors made by the animals before operation were not distributed randomly throughout the visual field. For each animal more errors occurred to a stimulus presented above the fixation point than below it, and more errors occurred when the stimulus appeared in the right upper quadrant than the

TABLE II
NUMBERS OF TRIALS AND PERFORMANCE SCORES FOR EACH ANIMAL
IN PERIMETRIC TESTING

Condition	Animal	Trials	"False Positives"	"False Negatives"
Pre-operative Bright stimulus	C-1	2850	44 (1.5%)	98 (3.4%)
	N-1	2600	96 (3.7%)	95 (3.7%)
	S-1	2555	49 (1.9%)	80 (3.1%)
	D-1	2638	15 (0.6%)	22 (0.8%)
Post-operative Bright stimulus	C-1	4515	79 (1.7%)	244 (5.6%)
	N-1	3487	215 (6.1%)	150 (4.3%)
	S-1	4942	55 (1.0%)	119 (2.4%)
	D-1	5393	113 (2.1%)	509 (9.4%)
Post-operative Dim stimulus	C-1	2579	60 (2.3%)	175 (6.7%)
	N-1	1448	176 (11.5%)	95 (6.6%)
	S-1	2179	88 (4.0%)	124 (5.7%)
	D-1	1168	57 (4.9%)	149 (11.9%)
Post-operative Dim stimulus White background ..	D-1	1457	19 (1.3%)	313 (21.5%)

Percentages are given to the first decimal place.

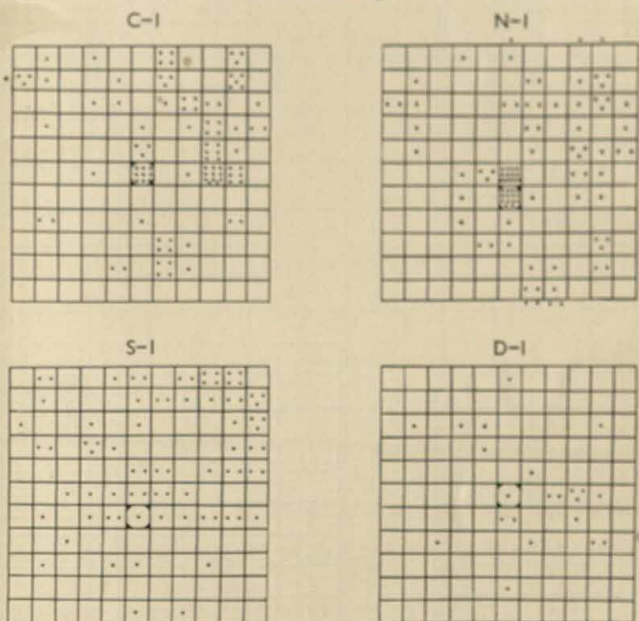
left. The errors made by each animal are shown in Figure 5. Each cell in each chart represents an area of the visual field 5 degrees across. The errors are referred to a common fixation point which is the heavily outlined cell in the centre. One further explanatory point is needed. The central stimulus bulb, which was outlined in white, was flashed eight times as often as any other before operation in an attempt to induce the animals to look at it. The fact that animals C-1 and S-1 made a relatively large number of errors in response to a centrally presented stimulus is therefore not surprising; and the number of correct responses to centrally presented stimuli was correspondingly greater. The other two animals rarely responded incorrectly to such a stimulus however often it was presented.

(2) *Post-operative testing.* The animals were tested for the first time 1 week after operation and the bright stimulus was used for several thousand trials. The scores are shown in Table II. Only animal D-1 showed any striking change. However, the distribution of "false negatives" is more informative than their total percentage and these are shown in Figure 6. It is clear that the first three animals made most of their errors when the stimulus fell on the macula and that animal D-1 had an impairment which involved the right half of his visual field. In every case an analysis of the visual stimuli which were *correctly* reported revealed that the deficit was far from

absolute. For instance animal C-1 showed the greatest evidence of a central field defect. It was known that this animal usually looked at the centre of the perimeter during testing (see below) and the eye photograph was accordingly examined for every trial on which the central stimulus bulb or one of the eight surrounding it was presented. This investigation revealed that when a stimulus fell within the area shown to be defective in Figure 6, the animal responded correctly about six times as often as he did not. With N-1 and S-1 the deficit would be even less severe.

An analysis of correct responses to stimuli appearing on the defective region was made in more detail with animal D-1. It was known that after operation this animal

FIGURE 3

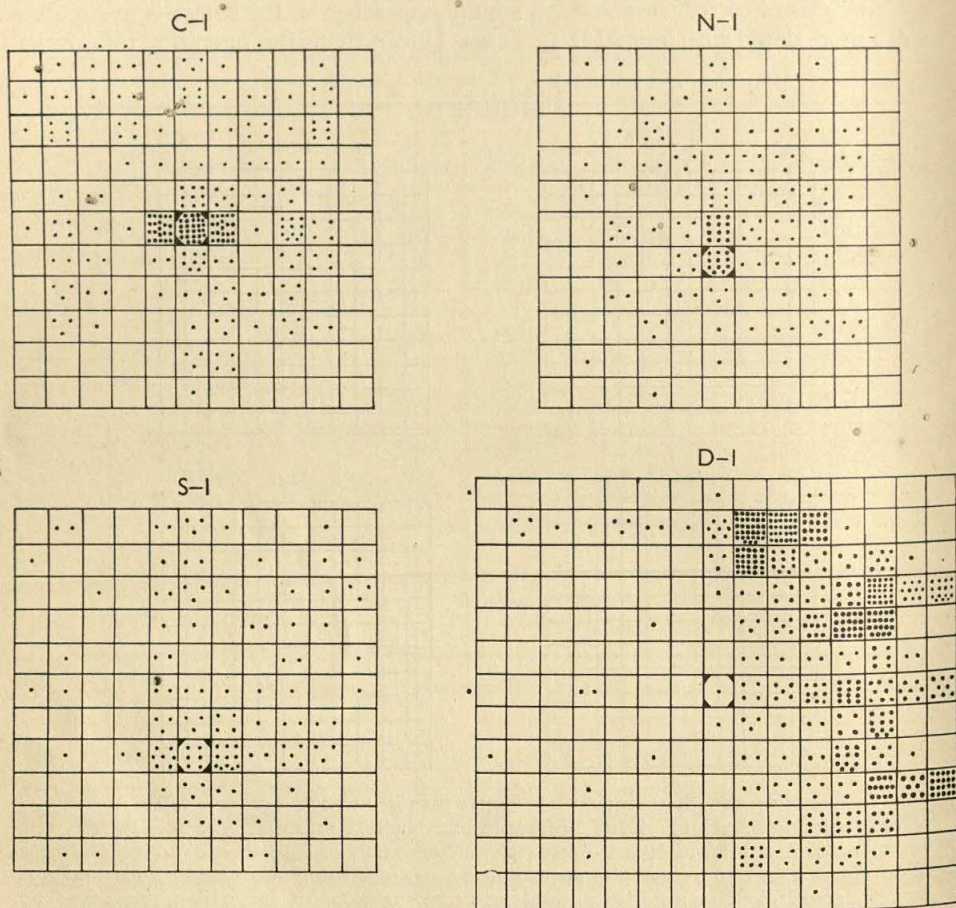


Charts showing the distribution of visual stimuli which led to "false negative" responses before operation. Each dot represents such a stimulus and is referred to a common fixation point which is the heavily outlined cell. The cells are 5 degrees apart. Dots outside a chart represent the approximate loci of stimuli whose position could not be found accurately because the animal was fixating a point for which there was no reference photograph.

almost always looked at the centre of the perimeter or at some point to the right of it (see below). Every trial in which a visual stimulus was presented in the right half of the perimeter and in which the animal responded correctly was therefore studied. With some of these trials it was found that the stimulus had appeared in the normal side of the visual field as a result of the animal looking to his right in the perimeter, but in the large majority of the trials the stimulus had appeared in the impaired field. The distribution of the latter stimuli was plotted with respect to a common fixation point and the results are shown in Figure 7. In each cell, the number in the top left corner represents the "false negatives" attributed to stimuli in that part of the field. This part of the chart is identical with Figure 6. The number in the bottom right

of each cell refers to the stimuli which appeared there and were correctly reported by the animal. The severity of the defect in any part of the field can therefore be assessed by comparing the two numbers. It can be seen that the further the stimulus lay from the fovea the greater the impairment appeared. In addition the upper quadrant of the field seemed more impaired than the lower.

FIGURE 6



Post-operative distribution of misreported visual stimuli when the bright stimulus was used.

One further point must be made about this data. Most of the errors using the bright stimulus occurred during the first 2 weeks of testing after operation. Thereafter they became progressively less frequent and had testing with the bright stimulus been continued indefinitely the ratio of total "false negative" errors to total correct responses would have increased to a point at which the field defect would seem slight, even with animal D-I.

For this reason the dim stimulus was now used until the end of testing. Animals C-I, N-I, and S-I had not encountered this stimulus before. The scores are shown in Table II and it can be seen that animal N-I showed some signs of abandoning a

difficult discrimination, for the percentage of "false positive" responses rose sharply and exceeded "false negatives." However, there was a definite grouping of misreported visual stimuli in the centre of the field with each of the first three animals (see Figure 8). This occurred despite the fact that the central stimulus bulb was presented no more often than any of the other bulbs used. A lengthy analysis was made of eye photographs to determine how often these animals responded correctly to stimuli which fell on and around the macula and the results are portrayed at the

FIGURE 7

Right of centre { Incorrect 472
Correct 621

								1		2							
	3	1	1	3	2	1	7	33	30	16	1						
								3		5							1
							3	25	11	5	3	8	1				
								6	4			1	3				
							1	2	4	4	9	25	10	15			
								2	6	11		4					1
				1				3	5	10	20	19					
								19	18	13	20	8					
								2	1	2	3	6	2				
								15	27	13	15	12					
			2				1	4	5	9	12	8	7	8			
								13	20	11	14	7	5	4			
							1	3	1	2	4	11					
								7	30	24	6	10					
1					1			2	1	2	11	5	1				
								20	33	11	19	6					
			1					2	3	3	3	13	8	20			
								8	22	21	26	10	1	2			
								1	2	6	9	8					
								1	4	9	9	1					
			1				1	9	3	3	5	2					
								6	6	17	20	10	1				
										1							

Animal D-1; bright stimulus. Distribution of all misreported visual stimuli (top left in each cell) and those correctly reported visual stimuli which fell within the field defect (bottom right in each cell).

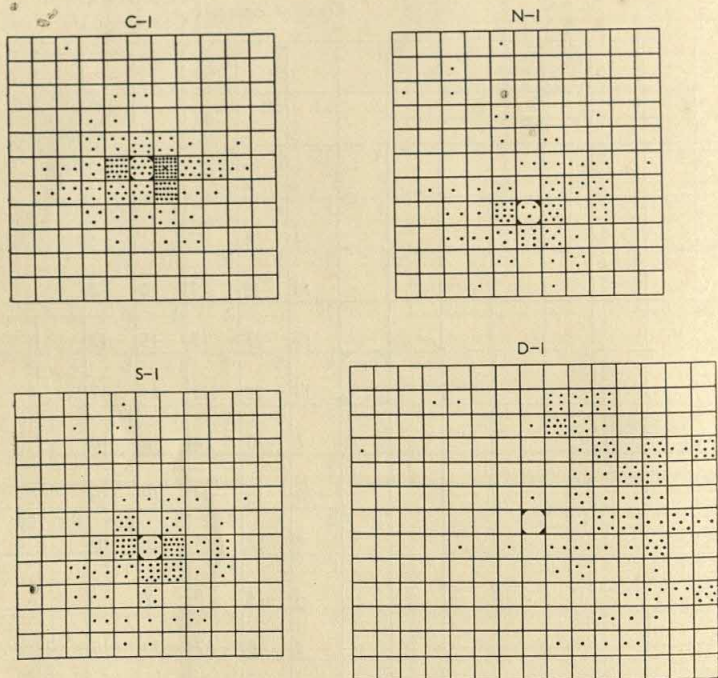
top of Figure 9. The numbers of incorrectly and correctly reported visual stimuli have been shown together, as in Figure 7, and it is clear that although a defect of expected size, shape and position was found it seemed far short of being a scotoma. The closest approach to a blind area seemed to be 5 degrees to the right of centre in C-1.

A similar result was obtained with animal D-1 and the distribution of stimuli which led to errors and to correct responses is shown at the bottom of Figure 9. A final series of trials was conducted with this animal after the surface of the perimeter was painted white. The dim stimulus was used and under these conditions it appeared extremely insignificant to a human observer. The results are shown in Figure 10; the field defect is obviously severe especially in the upper quadrant, but

it is still not absolute. The number of errors to the right of the common fixation point is 297 and the number of correct responses is 112, which is still high.

The distribution of visual stimuli which led to errors and to correct responses is a function not only of an animal's defect but also of the placing of the stimulus bulbs in any sequence of trials. For instance, if the animal always fixated the same point (and some of them approached this) the error distribution would slavishly follow the configuration of stimulus bulbs in the perimeter. The uniselector had only 63 positions and 32 of these were allotted to visual stimuli. Most of them had to be presented

FIGURE 8



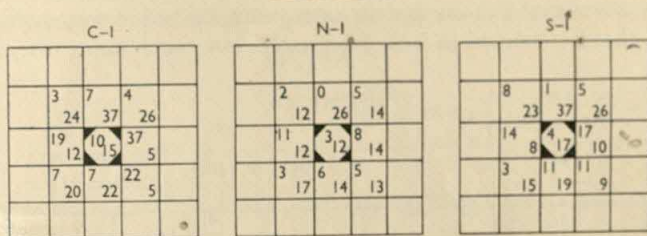
Post-operative distribution of misreported visual stimuli when the dim stimulus was used.

in the animal's intact field in order that his discrimination would not break down as a result of excessive errors. Therefore the number of points in the defective field which could be studied at any one time was small. For this reason the distribution of "false negatives" and correctly reported stimuli in the preceding figures is never uniform. Figure 11 partially overcomes this for D-I. All errors and all correct responses to a stimulus appearing in the impaired field which the animal made after operation are shown together and it is evident that the field defect extended to all the positions which could be tested. This method of presentation does not overcome the fact that some field positions were tested soon after operation, when the animal's performance was poorest, and other much later, when his performance had improved.

The ability of each animal to detect a flash of light which fell in the impaired part of its field might easily be explained if a pupillary response occurred to the stimulus and the animal used this response as a cue. This seemed highly unlikely in view of the size and intensity of the stimulus, but it was nevertheless tested with D-I. Both

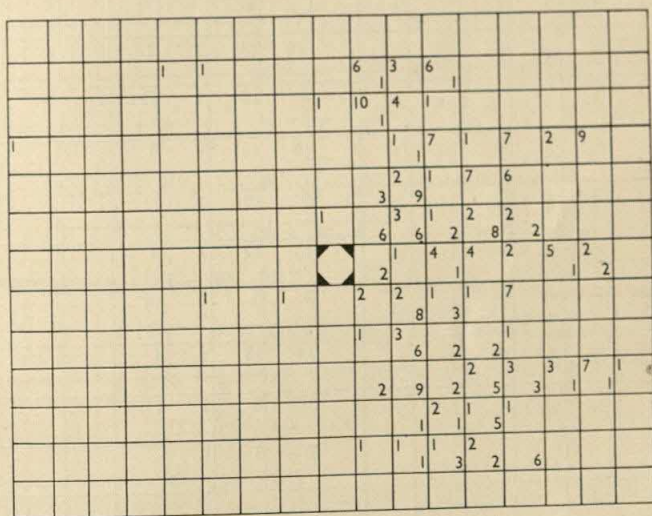
pupils were dilated and immobilized with a 2 per cent. solution of homatropine 40 min. before each of two testing sessions. Five hundred trials were given with the bright stimulus. The animal's performance was not altered in any way by this treatment and the distribution of detected and undetected stimuli remained what it had been

FIGURE 9



D-I

Right of centre { Incorrect 142
Correct 109

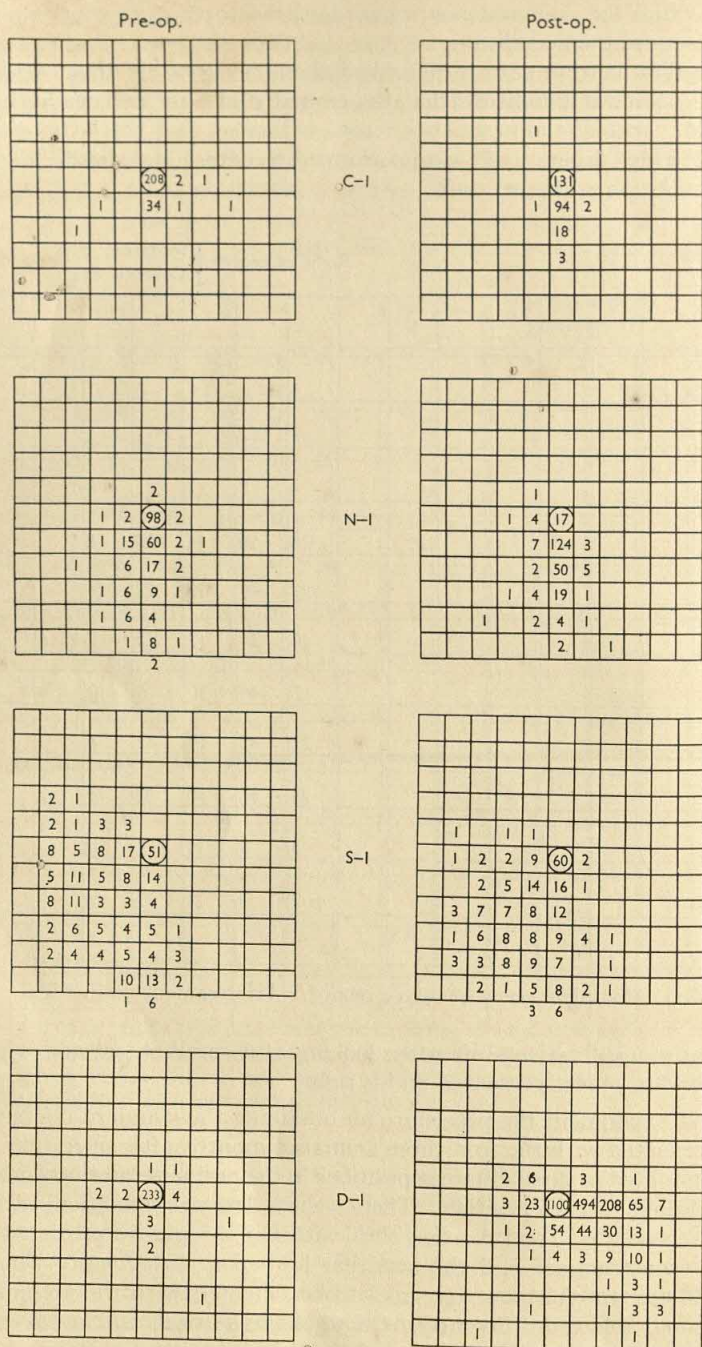


Distribution of all misreported visual stimuli and those correctly reported visual stimuli which fell within the field defect when the dim stimulus was used. Arrangement as in Figure 7.

when this stimulus was last used. It is therefore certain that the animal was not responding to a change in pupil size or to some other event consequent upon it.

(3) *Fixation.* It was stated earlier that an attempt was made to control the fixation of the animals during training. The charts on the left of Figure 12 show how successful this was. In this figure the cell embracing the circle represents the centre of the perimeter and it is fixation points, not unseen stimuli, which are referred to it. The numbers in each cell indicate how often the animal looked there in a total of 250 trials in which the eye photographed was suitable for analysis; otherwise the trials were selected randomly. The charts show that D-I almost always fixated the centre of the perimeter before operation, C-I usually did, and N-I and S-I were more variable. They rarely looked above the centre; to do this may have been mildly

FIGURE 12

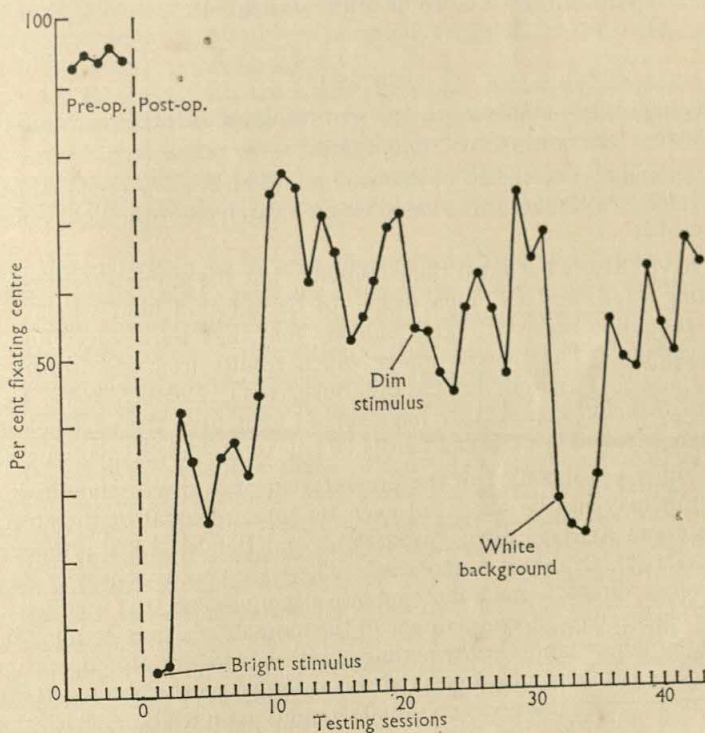


Charts showing the pattern of fixation of each animal during normal testing before operation (left) and after operation (right). The cell containing the circle represents the centre of the perimeter. The numbers show how often the animal looked there in a total of 250 trials or, with D-I post-operatively, 2,100 trials.

repeated with this animal using 11 different points in the perimeter. Thirty photographs were taken for each of seven of the positions and 60 for each of the remaining four. Out of 450 photographs the locus of fixation was found to be 5 degrees below the peanut in 308 of them. It coincided with it in only 23 and on 60 occasions a confident decision would not be made between the two. On all other trials the animal was either looking elsewhere or the eye was moving. It is concluded that after operation this animal fixated eccentrically by about 5 degrees.

The second experiment was performed with D-1 12 weeks after operation. A 6-in. carrot was mounted inside the perimeter 20 degrees to the right of the centre. Previous eye photographs had shown that the animal never fixated as far to the right as this

FIGURE 13



Graph showing how often D-1 fixated the centre of the perimeter before operation and throughout the testing period afterwards. The post-operative record involves the same trials used in Figure 12. Since the animal's performance varied after operation the number of photographs analysed in each session was not the same. The smallest number examined in any Session was 30, the largest 84, the mean 49.

except immediately after operation. Fifty trials were given in the normal way and nothing unusual occurred. The animal was then taken out whilst the carrot was moved to a similar position on the left. Testing was resumed and the animal reacted the moment he placed his eye to the peep-hole. He continually looked to his left, or rolled his eyes, and chattered. A further 50 trials were completed with difficulty although normally 200 trials would be accomplished without a break. At the next

testing session a small 6-in. white furry model monkey was used instead of a carrot. The monkeys in the colony were invariably terrified when this object was placed against their home cage or transport cage. It was first mounted between 15 and 20 degrees to the right in the perimeter and for 50 trials the animal showed no reaction to it. It was then transferred to the left. The animal placed its eye to the peep-hole, vocalized loudly, withdrew its head, and did not return. No further trials could be obtained but from his position behind the perimeter the experimenter could see the animal looking at the object from a position several inches behind the peep-hole. Clearly the animal failed to recognize either object when it lay to his right within the impaired part of the field, even though one object was white and contrasted sharply with the black surface of the perimeter. Probably he failed to see it, let alone recognize it for had he done so, however unclearly, he would presumably have explored the highly novel object with the intact side of the visual field.

DISCUSSION

A discussion of the validity of the present results must precede a consideration of their significance. Several points are relevant.

(1) An animal had no means of knowing on which trials a visual stimulus would appear therefore his response was based on an awareness of the light and not on an expectation of it.

(2) We have already excluded the possibility that the animals were responding to a constricting pupil, and the high degree of background light which was always present on the retina makes it highly unlikely that they were perceiving stray light from the stimulus. The blurred image which results from the imperfect optical system of the eye (Boynton, Enoch and Bush, 1954; DeMott, 1959; DeMott and Boynton, 1958) might conceivably have spread light over the edges of the central defects which we found but this can hardly be true of the defect involving a half-field.

(3) One other possible clue to the presentation of a visual stimulus is the blink reflex, for this may not be abolished even by total removal of the striate cortex. (Pasik, Pasik and Krieger, 1959; Pasik, Pasik and Bender, 1962) although Marquis and Hilgard (1937) felt that it is abolished. There are two reasons why this explanation is unlikely. Firstly a much more intense stimulus than that used here is needed to elicit the blink reflex. Secondly none of the animals was seen to blink in response to the bright or dim stimulus in the perimeter either before or after operation. Intermittent blinking occurred, of course, and was recorded on the film but it appeared as often on trials on which there was no visual stimulus as on trials on which the stimulus was present.

(4) The optic disc is totally blind yet our perimetric testing did not reveal it in monkeys. The distribution of unseen stimuli provides some evidence of the blind spot, e.g. Figure 8, but only a little. It is a legitimate inference that the field defects we studied were totally blind but that we could not demonstrate this for the same reason that we could not display the natural blind spot. We recognize this possibility but must discount it. Since the bright stimulus was faintly visible to a human observer when it fell on his optic disc, presumably because of scattered light, we might expect the same to occur with a monkey. The dim stimulus was not seen by human beings when similarly presented. However, it had to be very precisely aligned on the disc and this would rarely occur with an animal whose fixation was not controlled rigidly. Moreover the animals tended to look towards the centre of the perimeter and in this condition there was no stimulus bulb suitably placed to reveal

the blind spot which lies slightly *below* the horizontal meridian and nearly 20 degrees to the side (Cowey, unpublished observation based on a method of optically projecting the fovea and disc upon a tangent screen in front of the animal and confirming their position electrophysiologically). For this reason one would expect little evidence of the very small natural blind spot whichever stimulus was used.

(5) One last way in which an animal might correctly report a visual stimulus which he had not seen is by guessing, i.e. by pressing the lever appropriate to the light although he had not seen a light. However, the animals had no means of distinguishing between no-light trials, which formed 50 per cent. of all trials, and the much smaller percentage of trials on which the visual stimulus appeared but was not seen. This means that however often he were to guess that a light had appeared he would usually be wrong and the response would be recorded as a "false positive." If the animal were behaving in this way the number of "false positives" ought to be many times greater than the number of occasions on which he responded correctly to a stimulus falling within the field defect.

This was not the case, with the single exception of N-1 when the dim stimulus was used. For instance D-1 made only 19 "false positive" responses when the dim stimulus was used against the white background yet he responded correctly on 112 occasions when the stimulus appeared in the impaired half of the field (see Figure 10). It is therefore highly unlikely that the animal was responding correctly to the light by guessing.

The field defects which have been demonstrated are of interest in two major respects—their relationship to previous attempts to study them in animals and what they tell us about the organization of the visual system in monkeys.

It was pointed out in the introduction that in the past several different interpretations have been placed on the effects of operations comparable to those performed here. At least one thing is clear. A monkey may conceal even a very large field defect from an experienced observer, and the fact that defects are not obvious does not mean they are non-existent or slight. Presumably an animal becomes extremely adept at scanning the environment with his intact visual field even if the latter is quite small. However, it seems equally likely that he uses the sensitivity which remains within the defective region to alert him to visual events. Although the animal which lacks one occipital lobe may be unable to recognize or even be aware of a stationary object presented in the impaired field he can detect small changes in illumination and this may be sufficient to draw his attention to the majority of stimuli, e.g. moving objects, which previous workers have used in their attempts to display defective fields. Permanent change in fixation such as those postulated by Munk (1881), Dusser de Barenne (1933) and by Settlege (1939) may be unnecessary to account for the animal's behaviour and the defect may best be described as an amblyopia and not a scotoma, a view originally expressed by Loeb (1901) and Hitzig (1903).

The significance of the results to our ideas of the organization of the visual system is not so clear. The apparent absence of absolute field defects after damage to the striate cortex has been used as evidence for overlap within the retinostriate projection system (e.g. Klüver, 1927; Weiskrantz, 1961). But the abundant anatomical evidence of Minkowski (1920), Brouwer and Zeeman (1926), Polyak (1933, 1957), Clark and Penman (1934), Clark (1941), Glees and Clark (1941) and Glees (1961), lends no support to this hypothesis at any level of the retinostriate projection system. Nor does the electrophysiological evidence of Talbot and Marshall (1941), Daniel and Whitteridge (1961) and Cowey (unpublished observations). Electrophysiological overlap has been demonstrated in the cat (Doty, 1958) if responses of long latency are considered

but it has not been reported in the monkey. Moreover if overlap of projection fibres were to explain the residual vision of D-1 it would have to occur *between* the projections to the two occipital lobes and the projections are usually considered to be quite separate. There is electrophysiological evidence that lateral conduction of impulses occurs across the retina of the cat (Doty, 1958; Doty and Grimm, 1962) but unfortunately, it has not been studied in the monkey.

On the other hand there is some indirect evidence that the intact occipital lobe may contribute to residual vision. Klüver (1941) found that a monkey without any striate cortex could only respond to a single light if he was dark adapted and the light was bright. For instance there was no response to a stimulus light subtending 5 degrees by 3 degrees although the luminance of the stimulus was 250 millilamberts and of the background only 4 millilamberts. He had to reduce the background illumination to 0.325 millilamberts before there was a reliable response. Animal D-1, even though light adapted, could respond to a visual stimulus which was briefer, smaller and dimmer than Klüver's and which was presented in the impaired field. Klüver's animals appeared to be blind under such conditions.

A paradox exists here. If the impaired field in the "hemianopic" animal is functionally similar to the whole of the field in a destriated animal it is very odd that Klüver's animals were so impaired. Possibly the intactness of part of the visual field permits an animal to attend to the testing situation in a highly efficient manner. Klüver's animals had to do *everything* with very little visual guidance, which may explain why they were so poor. The alternative is to accept the difference between the impaired fields of an "hemianopic" animal and a destriated animal as genuine, in which case it is difficult to avoid the conclusion that the intact occipital lobe was contributing to the residual vision of D-1. If this is true the theory of point-to-point projection cannot be completely valid and an investigation of the means by which impulses from the left hemi-retinae can reach the right occipital lobe is justified.

It has not been demonstrated conclusively that a destriated monkey can respond to alternate black and white vertical stripes moving horizontally (ter Braak, 1936; Pasik, Pasik and Krieger, 1959). Nor has it been demonstrated that the superior colliculus plays an important role in this behaviour in the monkey, although the experiments of Smith and Bridgeman (1943) show that this region of the brain is crucially involved in optokinetic nystagmus in the guinea pig. Whatever the nervous mechanism involved in optokinetic nystagmus it seems profitable to explore its role in the residual response to small flashes of light. Optokinetic nystagmus is usually described as a response to movement but a moving pattern may change the illumination at any single retinal position in a manner comparable to a flash of light. The function of the superior colliculus is still ill-defined. A large number of optic fibres end in the superior colliculus of the monkey (Polyak, 1957). According to Brouwer and Zeeman (1926) the projection involves fibres from the peripheral retina only, but Whitteridge (1960) finds a response in the superior colliculus is most easily evoked by macular stimulation. If Whitteridge is correct this might help to explain why the hemiambyopia was least severe at and around the fovea and why the central defect of the first three animals was so difficult to detect.

We have concluded that field defects in monkeys are not absolute when caused by removal of striate cortex. This seems to indicate a difference in the organization of vision in man and monkey, and such a distinction has been drawn several times before (e.g. Dusser de Barenne, 1933; Marquis, 1935; Clark, 1942; Polyak, 1957). It would be incautious to stress this difference. There is some evidence that field defects in man may not be absolute (Bender and Krieger, 1951; Battersby, 1951; Poppelreuter, 1918) and that some sensitivity to light remains even when the defect embraces the

whole field and the damage to the geniculo-striate system is thought to be complete (Marie et Chatelin, 1915; Wilbrand and Saenger, 1918, cited in Teuber, Battersby and Bender, 1960). The only valid difference may lie in the relative severities of the field defects in man and monkey—it is the residual sensitivity which is difficult to demonstrate with man, it is the defect which is inconspicuous in the monkey.

Discussions which attempt a comparison of vision in man and in other animals usually omit to say what we mean by two of the terms most frequently used, namely *see* and *blind*. When we say that a human being *sees* a visual stimulus we mean that he had experienced something which most of us have felt ourselves and understood. If he is incapable of this experience we say he is totally blind; if visual stimuli fail to elicit the experience only when they are presented in certain parts of the visual field we say the subject has an absolute scotoma. If the subject can respond to a light only because it makes him blink or because it can be used as a conditional stimulus for blinking (Teuber, Battersby and Bender, 1960, p. 50) we nevertheless say he is blind for he can tell us that he did not *see* the stimulus. We are much less fortunate with animals for even if we infer that normally they may experience something similar to ourselves, we have no reliable indication of it. It is therefore extremely difficult to decide whether an operated animal's response to light indicates that he *saw* it or whether he is *blind* in the above sense of the word. Even if we exclude the possibility that the animal is using a blink reflex or a change in pupil size as a cue we must allow that the evoked response to light in the superior colliculus, the mesencephalic reticular formation, the cerebellum or even the pyramidal tract may provide the basis for a response to the light. The fact that direct electrical stimulation of the superior colliculus or the mesencephalic reticular formation can be used by an animal as a conditional stimulus (Nielson, Knight and Porter, 1962) supports this view.

The upshot is that if we do accept a response to light as evidence of sight and the absence of blindness, as numerous investigators have done since Goltz (1892) did so with the blink reflex to light in dogs, we may be talking about different things with the same names when we compare vision in man and animals. For instance, we mentioned above that man is supposed to be totally and permanently blind after striatectomy whereas the monkey is said to retain a primitive awareness of light; is it not conceivable that the monkey is much better equipped than man to utilize secondary effects of a visual stimulus as cues but that *seeing* a stimulus is organized in a very similar manner in the two species?

Our current knowledge prevents us from deciding whether the results presented in this paper ought to be interpreted in this way, but it is unwise to conclude that similar brain damage in man and monkey does produce dissimilar field defects when the defects are described in terms appropriate to clinical perimetry.

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INTRARETINAL TRANSFER AND THE FUNCTION OF THE OPTIC LOBES IN OCTOPUS

BY

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A method of training octopuses was developed which allowed the part of the retina stimulated to be controlled during training and subsequent transfer tests. Using this method the following results were obtained. (1) There was more stimulus generalization when the edge of the retina was stimulated than there was when the centre of the retina was stimulated, suggesting that discrimination is better with the centre of the retina. (2) Intraretinal transfer is more nearly complete than interocular transfer. (3) Continuity of the optic lobes between the projection areas of different parts of the retina is necessary if receptor transfer is to occur between them. The optic lobes are therefore involved in visual learning in the situation used.

INTRODUCTION

The following experiment was done for two purposes; (1) to find a situation for octopuses in which interocular transfer and transfer between different parts of one retina (intraretinal transfer) can be compared, and (2) to analyse the function of the optic lobes in visual learning.

The training situations which have been used in visual learning in the octopus to date all involve the animals attacking stimulus objects, and receiving rewards or shocks for doing so. Since the animals usually use only one eye at a time in attacking, and often rotate so that the image of the object being attacked falls upon a fairly localized area towards the centre of the retina of that eye, such training situations cannot be used to stimulate one part of the retina during training and another part of the retina afterwards in testing for transfer. A situation is required in which the animals do not move, so that the part of the retina stimulated can be both localized and controlled. This was achieved by showing the animals neutral stimulus objects, which they were not likely to attack with short latency, and giving them an electric shock in the home after 5 sec. The effect of the shock was thus conditioned to the stimulus object without any movement being made by the animals. After several such pairings it was usually found that the animals would not attack a crab when the stimulus object was shown at the same time, although crabs shown alone were still attacked. The strength of the association between any stimulus object, shown either to that part of the retina stimulated during training (the trained part of the retina) or to some other part of the retina (an untrained part of the retina), could then be measured by the extent to which attacks on crabs were prevented or slowed down when the stimulus object was shown at the same time.

Intraretinal transfer and interocular transfer are both examples of receptor generalization, in which the animals are trained with the stimulus reaching one part of the receptor surface, and then tested to see how far the learnt behaviour survives when the stimulus reaches another part of the receptor surface. A second type of generalization, stimulus generalization, was also tested in the present experiment. In stimulus generalization the animal is trained with one stimulus object, and then tested to see how far the learnt behaviour survives when the animal is presented with another, different, stimulus object. In the present experiment the animals were trained using a single stimulus object presented to one part of the retina, and stimulus

generalization was then tested while the animals were using the trained part of the retina, and also while they were using an untrained part of the retina. This is analogous to an experiment reported in Muntz (1962) on interocular transfer, except that in the present experiment both interocular and intraretinal transfer were tested.

The experiment also attempted to analyse the function of the optic lobe in visual learning. Removal of the lobes can give no information on this since the operation makes the animals blind. In tactile learning, when this problem does not arise, the optic lobes have been shown to be unnecessary for learning (Wells and Wells, 1957). Two attempts to approach the problem have been made using visual learning; in one slashes were made in the optic lobes (Boycott and Young, 1955), and in the other the animals were trained monocularly and the optic lobe of the trained side then removed (Muntz, 1961c). Neither of these operations had any effect on the animal's performance. In the present experiment the projection area of the anterior part of the retina on the optic lobes was separated from that of the posterior part of the retina by a vertical knife cut in the centre of the lobe, and receptor generalization between these two points then tested. In this way it was hoped that some light might be thrown upon the function of the optic lobes, by showing whether continuity between the projection areas of the receptor surface is necessary if receptor generalization is to occur between them.

The anatomy of the central nervous system of the octopus, in as far as it relevant to the present experiment, is shown in Figure 1. Each retina projects directly onto the optic lobe of the same side. The front of the retina projects to the front of the optic lobe, and the back of the retina to the back of the optic lobe, but there is inversion in the vertical plane so that the top of the retina projects to the bottom of the optic lobe and vice versa. Since the visual fields of the octopus have a slight overlap both in front and behind (Heidermanns, 1928), the front edges of both retinas can be stimulated by presenting a stimulus object centrally behind the animal, and the back edges by presenting a stimulus object centrally in front of animal. The experiments were concerned with transfer between these two points, and also with interocular transfer.

METHOD

Subjects. The subjects were 20 *Octopus vulgaris* Lamarck, obtained from the Bay of Naples and weighing between 200 and 400 gm. They were kept individually in tanks, 100 × 60 cm., with a depth of water of 40 cm. Two bricks were placed parallel to each other in the centre of the tank, with a gap of approximately 2 in. between them. This formed the animal's "home," the animals remaining most of the time in the gap. In this position they were at least 25 cm. from all the walls of the tank, and a stimulus object could then be presented at any point in the animal's visual field.

Acclimatization. The animals were allowed several days to settle down after being brought into the aquarium. They were then tested with crabs; if these were not readily attacked the animal was discarded. The animals which were operated on (see below) were allowed several further days after the operation to recover.

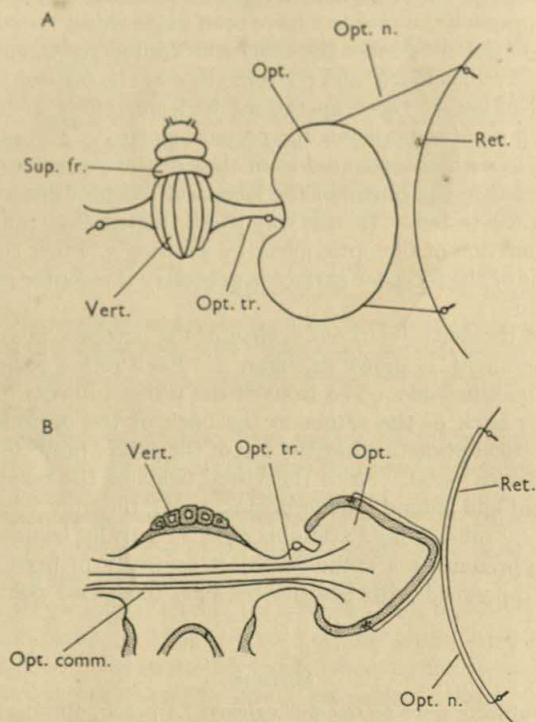
Initial training. One of two stimulus objects was used during training; a 10 × 2 cm. white vertical rectangle, or a horizontal white rectangle of the same dimensions. These were cut out of white plastic $\frac{1}{4}$ in. thick, and presented on a transparent perspex rod. The stimulus object used in training will be referred to as the positive stimulus object; the other rectangle, which was used in testing (see below), will be referred to as the transfer stimulus object. The stimulus objects were presented by hand, and were shaken in a vertical direction over a distance of about 3 in. at a rate of about three times per sec.

In the animals which were being used to test intraretinal transfer the positive stimulus object was presented either immediately in front of the animal, or immediately behind. Each animal was therefore trained on one of four combinations; horizontal presented in

front, vertical in front, horizontal behind, or vertical behind. At the end of 5 sec., or earlier if the animal started to attack, the subject was given an 8-volt electric shock in the home and the stimulus object immediately withdrawn.

The procedure with the animals being used to test interocular transfer was the same, except that the stimulus object was presented in the centre of the visual field of one eye. The four possible training situations for these animals were therefore the vertical rectangle presented to the right eye, the horizontal presented to the right eye, the vertical presented to the left eye, and the horizontal presented to the left eye.

FIGURE 1



Anatomy of the central nervous system.

A. View of the surface of the brain from above.

B. Transverse section through the optic tracts.

opt., optic lobe; *opt. comm.*, optic commissure; *opt. n.*, optic nerve; *opt. tr.*, optic tract; *ret.*, retina; *sup. fr.*, superior frontal lobe; *vert.*, vertical lobe.

Two presentations of the stimulus object accompanied by shock were given daily for 2 days. On each day the presentations were separated by one hour.

Testing. After 2 days of training testing was started. Three combinations of stimulus objects were used during testing; crabs shown alone, crabs shown together with the positive stimulus object, and crabs shown together with the transfer object. Each of combinations was presented to the trained part of the retina, and to the untrained part of the retina, making six possibilities in all. The animals were tested with all six possibilities of twice daily, once in the morning and once in the afternoon. Each set of six tests will be referred to as a testing session. The different combinations which formed the six tests were given in a random order, except that the two tests with the crabs shown alone were always given first and fourth. The tests were separated by 5-min. intervals. On each test the latency of attack was measured with a stop-watch, and if there was no attack

within 20 sec. the stimulus object and crab were withdrawn and the trial recorded as a failure to attack. The criterion of an attack was that the animal moved its body out of the gap between the bricks towards the stimulus object. The octopuses were never allowed to seize the stimulus object or the crab, which were withdrawn on each occasion as soon as the animal started to attack.

Retraining. After a variable number of testing sessions the association between the stimulus object and the shock weakened, and attacks were made on every test. When this happened two further pairings of the positive stimulus object and the shock were given, 5 min. after the first testing session on which attacks were made within 20 sec. on all six trials. The two retraining trials were themselves separated by 5 min. This process was repeated three times with each animal, so that there were four series of testing sessions separated by three retraining sessions. Some of the animals (three animals of Group 3 and one animal of Group 1, see below) escaped from their tanks and died before all three retraining sessions could be given.

Operations. In eight subjects the optic lobes of each side were bisected with a vertical knife cut, dividing the front of the lobes from the back. The operations were carried out under 4 per cent. urethane in sea water. The skin and muscles over the orbit were cut, and the white body reflected exposing the optic lobe. This was then divided, the white body replaced, and the incision closed by a single stitch through the muscle layer beneath the skin. The lobes of each side were cut consecutively during one period of anaesthesia.

Before training was started with these animals, they were tested with crabs presented in a variety of positions to ensure that the operations had left no appreciably large blind spots. This in fact never occurred.

Histology. The brains were stained with a modification of Cajal's silver stain (Young, 1939), and sectioned serially at 30μ . The extent of the lesion was reconstructed from the sections, and plotted onto a standard diagram.

Experimental design. The animals were divided into three groups, as follows:—

Group 1 contained four unoperated animals which were tested for interocular transfer. One animal was trained on each of the four possible combinations of one of the two white rectangles being presented to one of the two eyes.

Group 2. This group contained eight unoperated animals. Two animals were trained on each of the four possible combinations of one of the two white rectangles being presented centrally in front or centrally behind the animals. They were then tested for intraretinal transfer.

Group 3 contained eight operated animals. They were treated exactly as Group 2.

RESULTS

(1) *Effectiveness of the training procedure.* After initial training each animal received tests until attacks were made on all six trials of one testing session. The same procedure was followed after each retraining session. Thus different animals received different amounts of testing. Table I shows the average number of failures to attack per animal for the three groups, and also the average number of testing trials given per animal (including those on which no attack was made): the differences are small and not significant. The training did not prevent the animals attacking the crabs shown alone; on only five occasions in all, out of 552 presentations, did an animal fail to attack a crab shown alone.

Table I also shows the average number of failures to attack, and the average number of testing trials which were given (for all three groups considered together) on each series of testing sessions. Here the differences were more marked; the third retraining session in particular was relatively ineffective.

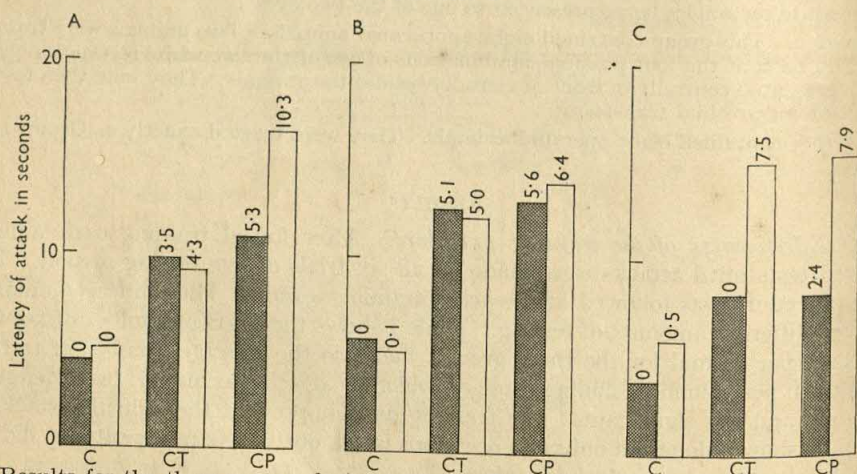
The results were analysed to see whether the number of tests received by an animal correlated with any of the scores considered below. There was no correlation, so that any effect that variations in the strength of the association between the stimulus object and the shock may have had on the generalization scores used was too small to show in this experiment.

TABLE I
EFFECTIVENESS OF TRAINING

	Average number of	
	failures to attack	testing trials
Group (1)	23.3	87.0
Group (2)	22.5	70.8
Group (3)	18.3	75.2
After initial training	6.1	21.0
After first retraining session	8.2	28.7
After second retraining session	7.3	26.2
After third retraining session	3.6	17.5

(2) *Results during testing.* These results are summarized in Figure 2. The columns in the histograms represent the average latency of attack for the different combinations of stimulus objects and part of the retina stimulated. The figures above the columns give the average number of occasions per animal on which no attack was made in 20 sec. These two measures correlate highly, but the average latency of attack would be expected to be a more sensitive measure since it includes information from the

FIGURE 2



Results for the three groups during testing. The columns represent average latency of attack, the figures above the columns the number of presentations per animal in which no attack was made within 20 sec.

□ = results for trained part of the retina.

■ = results for untrained part of the retina.

C = crabs presented alone, CT = crabs and transfer stimulus object presented together, CP = crab and positive stimulus object presented together.

(A) Results for interocular transfer (Group 1).

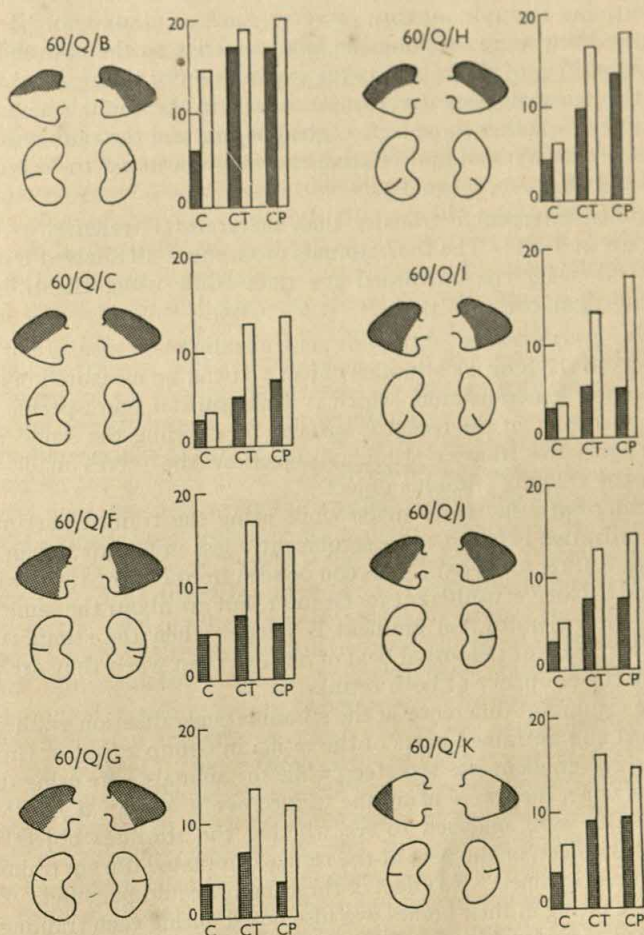
(B) Results for intraretinal transfer (Group 2).

(C) Results for intraretinal transfer with operated animals (Group 3).

60 per cent. of presentations on which attacks were made, which is not the case with the simple measure of whether an attack occurred or not. All statistics presented have therefore used this measure.

(A) *Receptor generalization.* The amount of receptor generalization occurring can be quantified by dividing the score obtained by the animals while they were using the untrained part of the receptor surface by the scores which they obtained while using the trained part of the receptor surface. In Figure 2 the degree of receptor generalization is therefore given by the length of the black columns divided by the length of the white columns.

FIGURE 3



Results for individual animals of Group 3. For each animal the lesion is presented on the left, and the testing results on the right. The top figure on the left represents a transverse section of the optic lobes; the extent of the lesion is coloured black. The bottom figure on the left represents a surface view of the optic lobes showing the position of the lesion on the horizontal plane. The lesions were vertical cuts in the lobes; only a very small mass of tissue was destroyed.

Conventions for the testing results as in Figure 2.

Intraretinal transfer is nearly complete in Group 2 (normal animals), the difference in scores obtained while using the trained and untrained parts of the retina being small and not significant (Fig. 2B). Group 3 (operated animals), however, showed significantly less receptor transfer than Group 2, there being no overlap between the two groups. The lesions were therefore effective in reducing the amount of intraretinal transfer (Fig. 2C). Nevertheless, some transfer was still occurring, for the average latency of attack in Group 3 was greater when one of the white rectangles was shown with the crab than it was when the crab was shown alone, even when the animals were using the untrained part of the retina. This residual transfer was probably due to the lesions being incomplete. The lesions for the individual animals are shown in Figure 3, together with their scores as measured by average latency of attack. It can be seen that some of the cuts in the optic lobes were not centrally placed. Even in the most extreme examples of this, however, such as animal 60/Q/I, a very large number of optic fibres were reaching the lobe posterior to the cut, and so it is not likely that the position of the lesions is the explanation of the residual transfer. An inspection of the figure suggests that transfer is least in the animals having the largest cuts, irrespective of whether these were central or not, and the rank order correlation between the size of the lesion and relative transfer was found to be 0.697, which is significant ($t = 2.38$, $n = 7$, $p < 0.05$).

There was less interocular transfer than intraretinal transfer, as can be seen from Figures 2A and 2B. The four animals of Group 1 all showed less effect from the training while using the untrained eye than while using the trained eye, the difference being significant ($t = 4.40$, $d.f. 3$, $p < 0.05$).

(B) *Stimulus generalization*. Stimulus generalization is a measure of how specific the training was to the positive stimulus object. It can be measured in an analogous manner to receptor generalization, which is a measure of how specific the training was to the trained part of the receptor surface, by dividing the scores made on the crab together with the transfer stimulus object by the scores made on the crab together with the training stimulus object.

If we consider only the scores made while using the trained part of the retina, stimulus generalization is found to be significantly less in Group 1 than in Groups 2 and 3 ($t = 2.55$, $d.f. 18$, $p < 0.05$). This can be seen from Figure 2; the white columns labelled CT and CP are very different for Group 1, but are about the same for Groups 2 and 3. Thus the generalization gradient is steeper when the stimulus objects are presented to the centre of the visual field of one eye than when they are presented so as to stimulate the periphery of both retinas.

There is no significant difference in the stimulus generalization gradients obtained with the trained and untrained parts of the retina in Group 2, but in Group 1 (interocular transfer) the gradient was less steep while the animals were using the untrained eye than it was when they were using the trained eye ($t = 5.47$, $d.f. 3$, $p < 0.05$).

(C) The results were analysed to see whether the stimulus object used as the positive stimulus object, or the part of the retina stimulated during training, had any effect on the scores obtained. No effect of these factors could be found. The direction the animals were facing in their homes was also noted during each training and testing trial; this varied considerably between trials, and no correlation between this and any of the scores could be found. Nor could any difference be found between the four series of testing sessions on either stimulus or receptor generalization scores.

DISCUSSION

(a) *Discrimination with the centre and the periphery of the retina*. The stimulus generalization gradient was found to be steeper when the stimulus object was presented

to the centre of the visual field of one eye than when it was presented in front or behind the animals. There was in fact little difference in the effect of the two stimulus objects in the latter case. In this experiment, therefore, the stimulus objects were discriminated more accurately with the centre of the retina than with the periphery.

From this result one might expect that the periphery of the retina would also differ anatomically from the centre. This has been shown to be the case. The cells are less deeply pigmented in the centre of the retina, and are also rather longer. Moving away from the centre the length of the cells decreases gradually, and they are also less densely packed at the edges. These changes occur more rapidly passing dorsally or ventrally from the centre than they do passing forwards or backwards, so that there appears to be a "fovea" along the equator of the retina in which the cells are relatively similar except at the extreme ends, which were perhaps the regions used in these experiments (Hess, 1905; Young, 1960).

(b) *Comparison of intraretinal and interocular transfer.* As stated in the Results, intraretinal transfer was more nearly complete than interocular transfer, there being in fact no significant difference (in the unoperated animals) between the performance shown using the trained and untrained parts of the receptor surface in the former case. Interocular transfer, however, was definitely incomplete. Thus in Group 1 the stimulus generalization gradient was significantly less steep while the animals were using the untrained eye than it was while the animals were using the trained eye, confirming results previously obtained (Muntz, 1962). It is not, however, possible on the basis of the present experiment to say whether the same phenomenon occurs in intraretinal transfer, for although the stimulus generalization gradient obtained with Group 2 using the untrained part of the retina is slightly less steep than that obtained using the trained part of the retina, both in terms of average latency of attack and in terms of the number of attacks prevented, there is very little gradient in either case and the differences are not significant.

In Group 1 (interocular transfer) the total effect of the type of training used in this experiment on the level of attack, obtained by averaging the effect of the two stimulus objects used, was less while the animals were using the untrained eye than it was while the animals were using the trained eye. This may be seen from Figure 2A; the white columns labelled CT and CP are, in total, longer than the black columns labelled CT and CP. This differs from the effect of positive training alone, which has an equal effect on the level of attack whichever eye the animal is using (Muntz, 1962). It therefore appears that, after monocular training, the effect of rewards on the level of attack is the same whichever eye the animal is using, but the effect of shocks is less while the animal is using the untrained eye. This agrees with the results of monocular discrimination training, in which shocks are associated with one figure, and rewards with another. Under these circumstances the level of attack is higher while the animal is using the untrained eye than it is while the animal is using the trained eye, which might be expected if the effect of the rewards transfers perfectly while the effect of the shocks does not (Muntz, 1961a). No explanation of this effect can at present be offered, but experiments reported in Muntz (1961b) make it unlikely that it is due to shocks being given to the same side of the body as the trained eye.

In this case also it is not possible to say whether the same phenomenon occurs with intraretinal transfer, although Group 2 does show a small effect in the same direction.

(c) *Function of the optic lobe in visual learning.* If cuts are made in a suitable position in the optic lobes, and the part of the retina which is used is controlled, then it is possible to demonstrate that the optic lobes are involved in visual learning. If, however, the cuts are placed at random, and the part of the retina stimulated is uncontrolled, behaviour is not affected, whether the cuts are made before or after

training (Boycott and Young, 1955). This suggests that even if the animal is trained using a small part of the receptor surface, the neural basis of the learnt behaviour (the engram) is nevertheless distributed throughout the optic lobes, and that after the engram has been formed any portion of the lobe will therefore be able to mediate the learnt behaviour. Thus, if, after the training, the stimulus is presented to an untrained part of the retina, the learnt behaviour will still be apparent; receptor generalization will have occurred. On the other hand, if the projection areas on the optic lobes of the trained and untrained parts of the retina are separated by a knife cut before training, then no engram is formed beyond the cut and so receptor generalization is prevented. If cuts are made after training, or if the cuts are made before training but the part of the receptor surface is not controlled, then the engram can still be formed throughout the optic lobes and behaviour will be unaffected.

Many experiments have shown that the vertical lobe is involved in visual learning (Young, 1961). Even without the vertical lobe, learning occurs in many situations, and the optic lobes are the most likely site for the engram in these circumstances. The present experiment provides direct evidence that this is so. It is possible that the engram is partially in the optic lobes and partially in the vertical lobe, both portions being necessary for learning, or that the engram is only in the optic lobes, but the vertical lobe is necessary for some other function, such as control of fluctuations in the level of attack, which is necessary if certain types of learning are to be effective. The present experiment does not distinguish between these possibilities.

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DECREMENT OF VISUAL AND HAPTIC MÜLLER-LYER ILLUSION ON REPEATED TRIALS: A STUDY OF CROSSMODAL TRANSFER

BY

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Twenty adults, 10 male and 10 female, ranging in age from 22 to 54 years, were asked to set the centre arrowhead of a Müller-Lyer pattern for 80 consecutive trials. Ten of the subjects did so visually, while 10 blindfolded subjects made the settings haptically, i.e. by active palpation. Following the 80th trial, those subjects who had been making settings visually were blindfolded and were asked to make six haptic settings. Conversely, those who had made 80 haptic settings proceeded to make six settings visually.

The initial size of the illusion was essentially identical for the visual and haptic groups. Both groups showed significant decrement, since 19 of the 20 subjects had a smaller illusion on trials 71-80 than on trials 1-10. There was significant transfer of the decrement to the six settings made in the other modality. However, transfer effects were greater for the subjects who went from touch to vision than for those who went from vision to touch. Verbalization did not appear to be the vehicle of these crossmodal transfer effects.

INTRODUCTION

If one looks repeatedly at the Müller-Lyer pattern, the illusion decreases in size. Ever since the early reports of Judd (1902) at the turn of the century, this decrement has been obtained in many ways: with spaced trials (Köhler and Fishback, 1950), and with trials that were massed (Mountjoy, 1958a); with fixation (Köhler and Fishback, 1950) or freely moving gaze (Selkin and Wertheimer, 1957). It has even been demonstrated after prolonged inspection without repetition of judgements (Selkin and Wertheimer, 1957). Cessation of trials has led, in some experiments, to a further decrement in the illusion demonstrable when testing was resumed (Köhler and Fishback, 1950); others obtained, instead, a partial or complete return of the illusion (Mountjoy, 1958b). In all of these studies, decrements appeared—no matter whether the subjects knew about the illusion, or not. The illusion diminished in the absence of any differential reward or information.

Work on the decrement has thus covered a great many conditions of exposure and testing. Nevertheless, the range of conditions has been restricted in one important respect: the illusion has always been presented visually, and no attempts have been made to find out whether analogous decrements might appear in some other sensory modality. Révész (1934) and others have reported that the Müller-Lyer illusion can be obtained when the pattern is presented through the sense of touch. Would there be a tactual analogue of the decrement, i.e. would repeated palpation of a tactile Müller-Lyer pattern lead to a decrement in the extent of the illusion? And, if there are decrements of both visual and tactual illusions with repeated trials, would decrements transfer from one sense modality to the other?

METHOD

Subjects

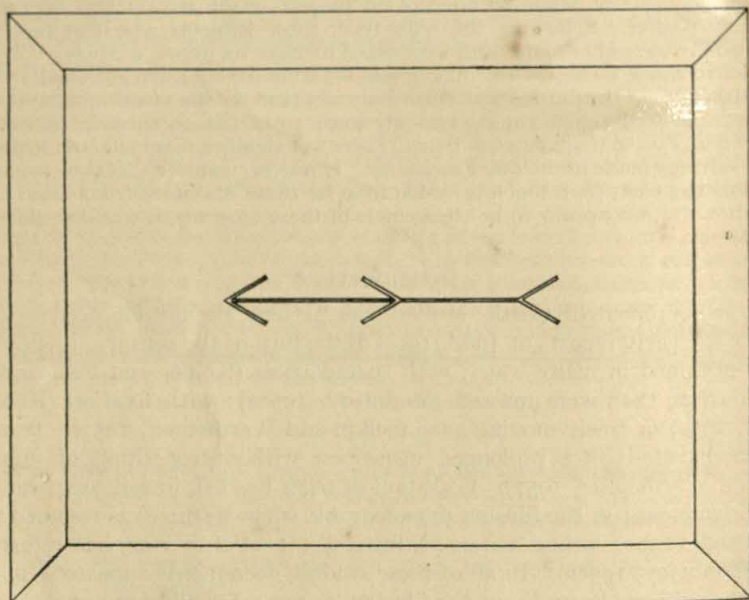
Twenty normal adults served as subjects, ranging in age from 22 to 54 years, with a mean age of 32. Ten were male and 10 female. Five males and five females were trained

on the visual task and then tested for transfer to touch; the remaining subjects were given tactual training and then tested for transfer to vision.

Equipment and Procedure

To equate the tasks as much as possible, the identical apparatus was used for touch and vision (Fig. 1). A metal rod, 2 mm. thick and 27 cm. long, was mounted on a plexiglass background which formed a rectangular surface 45×60 cm. This in turn was held by a picture frame. The metal rod was affixed to the plexiglass parallel to the long edges of the rectangle; the midpoint of the rod coincided with the centre of the rectangle. At each end of the metal rod was an arrowhead bent at a 60° angle, each side 4.4 cm. in length. The two arrowheads at each end of the rod were fixed in place; their apices pointed to the subject's left. By means of a hole through its apex, a third arrowhead of exactly the same size and shape was rendered movable and could traverse the rod. The apex of this

FIGURE 1



Müller-Lyer pattern used in visual and haptic tests. For details, see text.

movable arrowhead pointed to the subject's right. A centimetre ruler on the back of the plexiglass made it possible for the experimenter to read, from behind, the point at which the centre arrowhead was set.

Visual training. The subject was seated opposite the experimenter, separated by a table 3 ft. wide. He first saw the apparatus with the centre arrowhead moved to one end, that is, with the wing tips meeting at his left. He was told that the experimenter would move the centre arrowhead until he (the subject) thought that its apex bisected the length of the arrow. When the instructions were clearly understood, the actual trials were begun. Correction was permitted if the subject said he thought the midpoint had been overshot. Length of time for each trial was recorded from the instant the centre arrowhead was set in motion until the subject indicated that the midpoint had been reached. To prevent him from basing his judgements on a fixed period of time, the centre arrowhead was moved at varying speeds. Immediately after a judgement had been made, the arrowhead was returned to its original position. The procedure was repeated 40 consecutive times. There was no fixation point, nor was there any fixed intertrial interval.

A second series of 40 trials followed a 5-min. break. This time the centre arrowhead was started from the opposite end of the total figure, i.e. with the apices of the arrowheads meeting, at the subject's right.

Haptic training. The haptic trials, so-called because both touch and movement are involved, were performed in the same way as the visual trials except that the subject moved the centre arrowhead himself. In this way, of course, he also controlled the time per trial. He was blindfolded before he entered the room and, while instructions were given, the thumb and index finger of his preferred hand were guided by the experimenter over the Müller-Lyer pattern. This guided exploration began by placing thumb and index finger of the subject's preferred hand, first, on the apex of the fixed arrowhead on his left. The experimenter then guided the subject's two fingers over the two wings of this fixed arrowhead, on the left, and thence along the entire length of the shaft, until his fingers were inside the movable arrowhead. This movable arrowhead had been moved by the experimenter prior to the subject's initial exploration, to the extreme right, so that its apex touched the apex of the fixed arrowhead at the right. The experimenter then lifted the subject's fingers onto the flaring wings of the fixed arrowhead on the right, taking care to have him appreciate their position and extent. He then placed the subject's fingers on the point where the apices of movable and fixed arrowhead met, and said, "This is the end of the rod."

The subject was then told that there was a part (of the equipment) that moved; his fingers were placed by the experimenter on the apex of the movable arrowhead, then guided over its wings. Still guided by the experimenter's hand, the subject was made to move this movable arrowhead over the entire length of the shaft, back and forth, several times. The subject was then told that his task would be to get the "point" (i.e. the apex) of the movable part, on every trial into the exact middle of the rod, that is, "exactly halfway between this point here and this point there." While saying this, the experimenter guided the subject's two fingers to the apices of the fixed arrowheads. At no time did the experimenter use terms such as "arrow," "arrowhead," "apex," or "shaft"; all instructions were given by using the words "points" and "lines" and by guiding the subject's fingers appropriately and concomitantly. The subject was told that he could check any given placement of the movable point in the middle of the rod by running his two fingers along the rod to the right and left, and to keep running his fingers over the entire pattern. However, he was forbidden to "span" any distances by moving his fingers apart, or by using more than the thumb and index finger of his preferred hand.

When the subject indicated that he understood the procedure, the centre arrowhead was set so that the wing tips touched those of the arrowhead at the end, at his left, and he proceeded to move the centre arrowhead on each trial until he indicated he believed it bisected the line. He was permitted to correct a given setting until he was satisfied. Length of time per trial was recorded. After each setting by the subject, the experimenter returned the centre arrowhead to the starting point. Forty trials were followed by a 5-min. break and 40 additional trials with the centre arrowhead now starting from the right.

Transfer trials. As soon as the subject had completed 80 visual or 80 haptic trials, he was tested for transfer. Those trained visually were immediately blindfolded and given six haptic trials. Those trained haptically had their blindfolds removed and six trials proceeded as in visual training.

All subjects were told that trials would be repeated "many, many times," but that this repetition had nothing to do with their success or failure. They were also told that they were being timed, but that they could take as little or as much time as they liked; that time had nothing to do with their performance. At the end of the experiment, after trial 86, each subject was asked "How do you think you did?"—a question vague enough, we hoped, to elicit information, without interviewer bias, on the subject's awareness of any change in his settings.

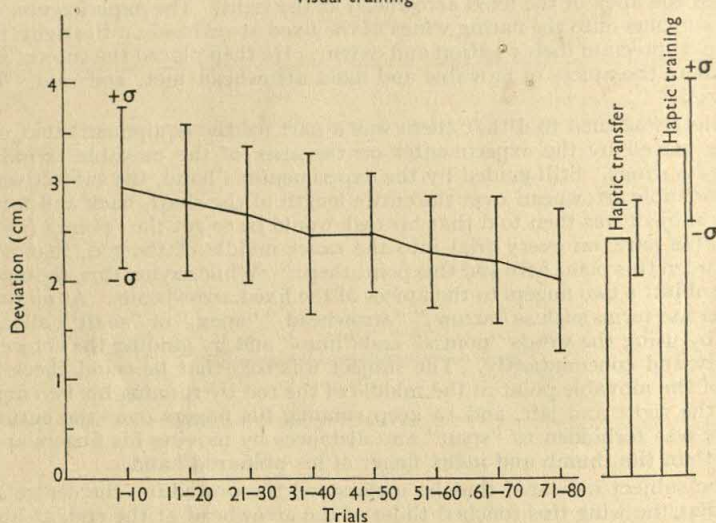
Control procedure. In order to assess effects of repeated settings *per se*, a supplementary group of five subjects was asked to bisect a visually presented line without arrowheads 80 consecutive times. Another five subjects did the same with tactual presentations. There were no consistent trends in the settings of these two control groups; some increased in accuracy with repeated trials, some decreased; neither of the control groups showed significant constant errors. Since there were no directional tendencies, these control groups will not be mentioned further.

RESULTS

There is no doubt from this experiment that the so-called optical illusion has its haptic analogue. At the beginning of the 80 "training" trials (i.e. the first 10 trials), visual and haptic illusions were practically identical in size. Further, the fate of the illusion appears to be the same whether repeated trials take the form of visual or haptic exposure. There is decrement in both cases (see Fig. 2 for the visual and Fig. 3 for the haptic effects). Evidently, the haptic illusion decreased somewhat more than the visual illusion, but this difference was not significant. While there were great individual variations in the initial size of the illusion (which ranged from 1.9 cm.

FIGURE 2

Visual training



Error (in cm.) of bisection of Müller-Lyer pattern (shown in Fig. 1) on 80 consecutive trials under visual control. Each point on abscissa represents the average setting by 10 subjects on a block of 10 trials. Vertical lines at each point indicate one standard deviation above, and one below each mean. The first histogram at the right represents the average of six settings made haptically by the same subjects immediately after their 80th visual trial. The second histogram (extreme right) gives the average of the first six settings made by another group of 10 subjects who underwent haptic training (in 80 consecutive trials). Note decrement from first to last block of trials, during visual training. The slight rise in the curve after the 40th trial may represent effects of a 5-min. rest period. Note transfer effect of the decrement to the six haptic tests, as compared with the extent of the illusion at the beginning of haptic training (extreme right).

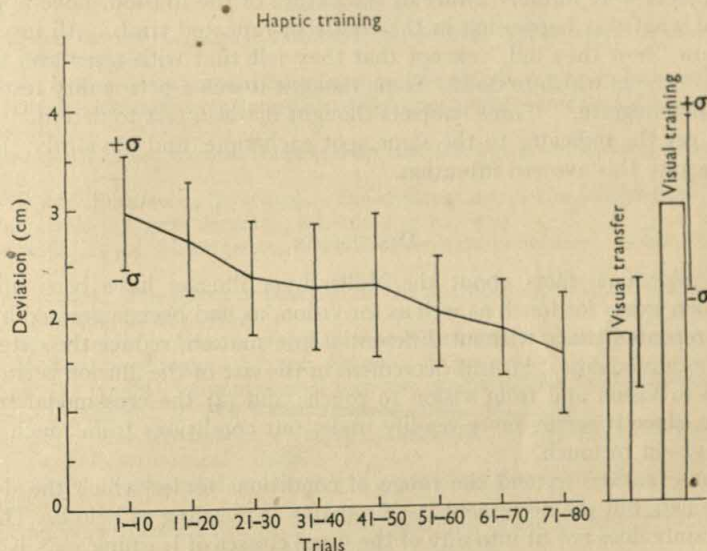
to 4.8 cm.), 19 of our 20 subjects had a smaller mean illusion, regardless of mode of presentation, during trials 71-80 than during trials 1-10. These similarities between modes of administration are all the more noteworthy, since the pattern was moved by the experimenter on visual trials, and by the subject himself, on the haptic trials.

These results were analysed by *t*-tests (and *t*-tests for correlated means, as appropriate). For the average of all subjects trained with visual exposure to the pattern, the decrement in the size of the illusion (measured by comparing the mean of the first 10 trials, 1-10, with the mean of the last 10 trials, 71-80), was significant at the 0.001 level. The same was true for subjects trained with haptic trials; the decrease in the illusion, from the first 10 (1-10) to the last 10 (71-80) trials, was significant at

the 0.001 level. No subject under our conditions of testing in 80 trials, achieved a consistent "negative" illusion. There were sizable differences in the rapidity and extent of the decrement, but there were no significant sex differences, whether assessed in terms of initial illusion, or in terms of course and extent of its decline. On haptic, but not on visual training, subjects with large initial illusions showed more decrement ($p < 0.01$) than those whose initial illusion was small. To determine this, the 10 subjects in each group were subdivided into the five with the largest initial illusion and the five with the smallest. The amount of decrement in each of these subgroups was then compared by *t*-test.

Transfer effects were measured by comparing the mean illusion on the six transfer trials (trials 81-86) with the first six training trials (trials 1-6) in the same modality. In other words, judgements of the 10 subjects on six visual transfer trials, after

FIGURE 3



Error (in cm.) of bisection of Müller-Lyer pattern (shown in Fig. 1) on 80 consecutive trials while blindfolded (haptic trials). For explanations, see legend for Figure 2.

80 haptic training trials, were compared with the first six judgements (trials 1-6) of the 10 subjects trained visually. Conversely, judgements of the 10 subjects on the haptic transfer trials (following visual training) were compared with judgements during the first six trials of the subjects initially trained haptically.

In this way, the initial judgements in any particular modality were compared with each other, before and after training in the other modality. If training in one modality does not affect performance in the other, there should be no difference between the transfer trials which follow training and the initial training trials in that modality. However, this is clearly *not* the case. Following 80 visual training trials, all 10 subjects made smaller errors in finding the midpoint of the Müller-Lyer pattern haptically than did those who made the judgements haptically without prior visual training. The transfer effect, estimated by comparing six haptic transfer trials with the first six training trials of subjects initially exposed to the haptic pattern, was significant at the 0.02 level.

Decrements also transferred from touch to vision. Subjects initially trained on

the visual pattern showed the illusion much more than did those who made the visual judgements after 80 haptic trials. Transfer effects from touch to vision were significant at the 0.001 level. Thus, transfer in both directions is significant, but the degree of crossmodal transfer from touch to vision is greater than that from vision to touch, a difference significant at the 0.01 level of confidence.

As can be seen from the bars indicating transfer on Figures 2 and 3, the judgements on transfer trials are not as close to the true midpoint as are judgements on the immediately preceding training trials in the first modality. Thus, there is some loss (return of the illusion) with shift to another modality; this loss was significant at the 5 per cent. level after visual, but not after haptic training. The gain (transfer) is pronounced, nevertheless, if it is assessed against what subjects tend to do in a particular modality without prior training in another.

There are several qualitative observations that should be mentioned. Whether or not our subjects were initially aware of the nature of the illusion, none appeared to be aware of what was happening in the course of repeated trials. All insisted they did not know "how they did," except that they felt that with repetition they took less and less time in which to do it. Some thought it was a personality test—"to see how fast I disintegrate." Three subjects thought it was a test to determine whether they could get the indicator to the same spot each time, and obviously, decrement occurred despite this avowed intention.

DISCUSSION

Certain additional facts about the Müller-Lyer illusion have been uncovered: (1) the illusion exists for touch as well as for vision, as had been suggested by Révész (1934); (2) repeated trials, without differential information, reduce the extent of the illusion in both modalities; (3) this decrement in the size of the illusion is transferable from touch to vision and from vision to touch; and (4) the crossmodal transfer is asymmetric, since it occurs more readily under our conditions from touch to vision than from vision to touch.

These observations extend the range of conditions under which the decrement can be obtained, but the decrement itself remains as puzzling as before. The phenomenon certainly does not fit into any of the usual classes of learning. As Köhler and Fishback (1950) have stressed, decrements appear without differential reward or information about the settings, so that it is not clear what is learned within a series of trials.

Whether the decrement is interpreted as a form of satiation (Köhler and Fishback, 1950), or as a form of progressive perceptual isolation of the test line (Judd, 1902; Lewis, 1908), or of "increasingly uniform density of encounters" with different parts of the figure (Piaget, 1961, p. 189), the crossmodal transfer of such decrements is not readily explained by any of these theories. Köhler (personal communication) is doubtful about the existence of crossmodal figural after-effects, such as those reported from our laboratory by Jaffe (1954). Held's recent studies of changes in perception on active exploration of atypical environments (Held, personal communication; Teuber, 1960) likewise disclose little evidence for intersensory adaptation. Yet, everyday perception of objects suggests a far-reaching congruence between vision and touch-kinesthesia.

Perhaps it is actually more surprising that so many recent studies have failed to demonstrate crossmodal transfer for various forms of perceptual learning (Cole, Chorover, and Ettlinger, 1961; Ettlinger, 1960) than that crossmodal effects should occur as they did in the present study.

The closest parallel to our results can be seen in an earlier report on intermodal pattern discrimination learning, from our laboratory (Krauthamer, 1959). In this experiment, there were marked crossmodal transfer effects between series of tactual and visual presentations of forms (with better transfer, again, from touch to vision than from vision to touch). The possibility remains that transfer, in Krauthamer's study, was mediated by verbal labels, but it is not readily seen how language could have served as a supramodal vehicle for transfer in the present experiment. As we pointed out, subjects were unable to tell what happened to their settings during the long series of trials; few were aware of the initial illusion, as such, and none were aware of its progressive diminution.

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SHORTER ARTICLES AND NOTES

THE DEVELOPMENT OF FINGER DIFFERENTIATION

BY

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The ability to perform discriminations based on the classification of sensory input from the fingers in terms of the individual fingers stimulated in their correct spatial arrangement, was tested in a group of normal children aged $4\frac{1}{2}$ to $7\frac{1}{2}$ years. The results showed that the acquisition of this ability depended upon chronological age in a regular fashion, suggestive of maturation of a unitary function ("finger sense") in a population subject to normal variation. The significance of this finding in the wider context of the acquisition of abilities during normal development is discussed.

The ability to move single fingers, leaving the others at rest, is acquired at the age of about 6 years (Rey, 1952). This skill must have its sensory counterpart. The ability to classify sensation from the fingers in terms of the particular finger from which it was derived ("differential finger sensation") ought to be detectable by appropriate testing. The four fingers, morphologically somewhat similar, are distinguishable chiefly on the basis of their positions relative to one another. In this respect they differ from other parts of the body, with the exception of the toes. The questions arise, at what stage in maturation does it become possible to perform discriminations based on the differentiation of sensations from the fingers, and does it at the same time become possible to succeed in tasks requiring an appreciation of their spatial arrangement?

To answer these questions, and to make it possible to detect the postulated abilities in a reliable manner, three new test procedures were designed. They are described below, and the results obtained presented.

PROCEDURE

1. *Finger differentiation test*

The subject faces the examiner, hand palm down, fingers spread, eyes closed. Two points on the fingers are simultaneously touched—sometimes both on the same finger and sometimes on two adjacent fingers (Fig. 1). The examiner asks: "How many fingers am I touching—one or two?" He maintains contact until and answer is given, and long enough after this to permit the subject to verify the answer under direct vision. The procedure is repeated eight times, four on each hand. Simultaneous touch is applied to the same finger and to two adjacent fingers in irregular sequence. Two trials involving the same finger were given with each hand, and two involving adjacent fingers.

The test is preceded by an illustrated explanation under vision—"now I am touching one finger—now I am touching two fingers—now I am touching how many fingers?"

The examiner does not embark on the test proper until he has ascertained that the subject understands what is required.

Six correct answers out of eight are accepted as a "pass."

2. *The "in between" test*

With the hand positioned as before, two fingers are simultaneously touched (Fig. 2). The examiner asks: "How many fingers are there in between the ones I am touching—how many in the middle?" He then maintains contact until answered, and long enough to permit the subject to verify the answer under direct vision.

The test is repeated eight times, four on each hand. The finger pairs are touched in irregular sequence, but such that the correct answer on each side should once be "0," twice "1," and once "2."

The test is preceded by an illustrated explanation while the subject has his eyes open. "Now there are two in the middle—now there aren't any—now there are how many?" The test proper is only begun when the task has been well understood.

Six correct answers out of eight satisfy the requirements for "pass."

FIGURE 1



Finger differentiation test.

3. The finger block test

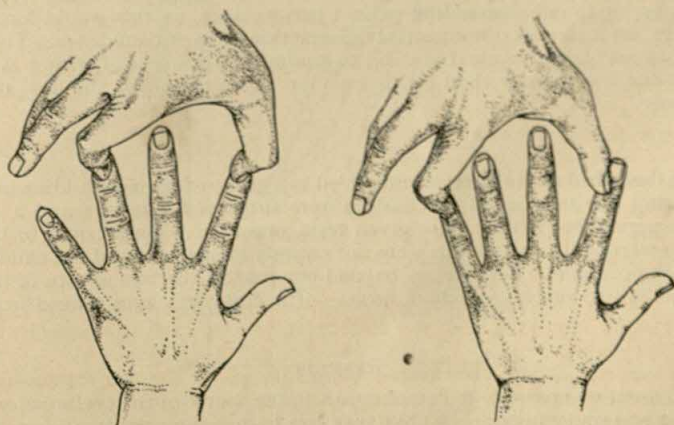
a. Four wooden blocks shaped as shown in Figure 3 are placed on the table. While his eyes are closed, the subject's fingers are moulded around one of four corresponding test blocks. He is then asked to open his eyes, and without looking at the block in his hand, to pick out the corresponding one on the table: "Which of these is the same as the one in your hand?" Having answered, he is permitted to verify his answer by direct visual comparison. This test is preceded by an illustrated demonstration under vision: "This block is the same shape as that one, this block is the same as which one?" When the examiner is satisfied that the task is understood, he proceeds to the test proper.

Each hand is used in four trials; each of the four blocks is used twice, once in each hand. Six correct answers out of eight are considered a "pass."

b. Test 3a involves cross-modal transfer ("touch-vision"). A modified form of the test which avoids this is as follows:—

While the subject's eyes are closed, two blocks are placed, one in each hand. The subject is asked: "Are these two blocks the same or not the same?" As before, he is

FIGURE 2



"In-between" test.

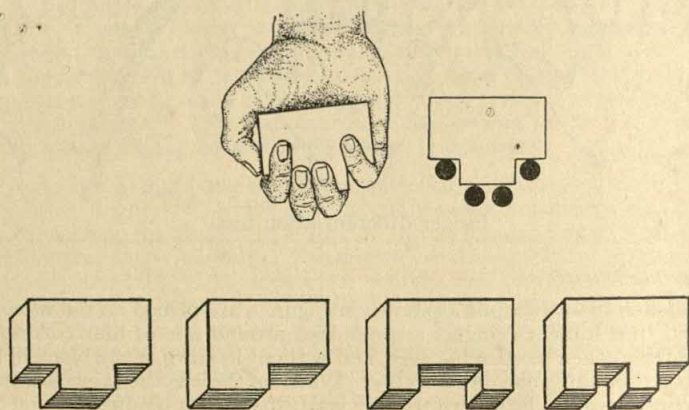
permitted to verify each answer under direct vision. The testing is continued for eight trials, identical and non-identical blocks being used in irregular sequence. In four of the trials the blocks are identical, in four they are not.

Six out of eight correct answers are regarded as a "pass."

The principles involved in the tests were as follows:—

Test 1 determines whether sensory input from the total surface of the four fingers is classifiable in terms of four distinctive finger areas, or whether the total finger mass is treated as a single undifferentiated source of sensory information.

FIGURE 3



The finger block test.

Blocks are 2 in. $\times$ $1\frac{1}{2}$ in. $\times$ $\frac{1}{2}$ in. with cut-outs $\frac{1}{2}$ in. $\times$ $\frac{1}{2}$ in. $\times$ $\frac{1}{2}$ in.

Test 2 can only be passed if the subject has at his disposal information as to the order of the fingers; if not, he will know how many fingers are being touched, but not how they are placed in relation to the ones that are not.

Test 3. Each of the four blocks is held in such a way that two of the four fingers are further flexed than the other two. But only if the subject knows how the four fingers are related to each other spatially can he discriminate between the four possible shapes.

The criterion for "pass" (six out of eight correct) is necessarily arbitrary. Nevertheless, it seemed superior to one demanding perfect performance, as this would have produced "false failures" in instances of momentary distraction or over-confidence. The use of the criterion of six out of eight made it possible to minimize this source of error. A pilot study showed that the use of more than eight trials did not often affect the final result in the individual case.

Subjects

The tests described above were administered to a group of normal children in an infants' school. During one test period 91 children were subjects for tests 1 and 2, and during another test period 89 children were given tests 3a and 3b. On account of the vagaries of school attendance the two groups were not entirely identical; 63 of the children participated on both occasions. For reasons beyond our control the two groups of trials had to be performed in two successive school terms. The children's ages ranged from $4\frac{1}{2}$ to $7\frac{1}{2}$ years.

RESULTS

In every case it was possible to demonstrate, in the course of the preliminary procedure, that each test was understood; each child was able to give the correct answers when tested under vision.

The number of children passing each test, grouped in four age groups, is given in Table I. The number, in percentage form, of children in each age group who passed each test is given in Figure 4. It is apparent that, in the population tested, the percentage of children to succeed on each test rose in a regular fashion with increasing chronological age.

TABLE I
RELATIONSHIP BETWEEN SUCCESS ON THE FINGER TESTS
AND CHRONOLOGICAL AGE

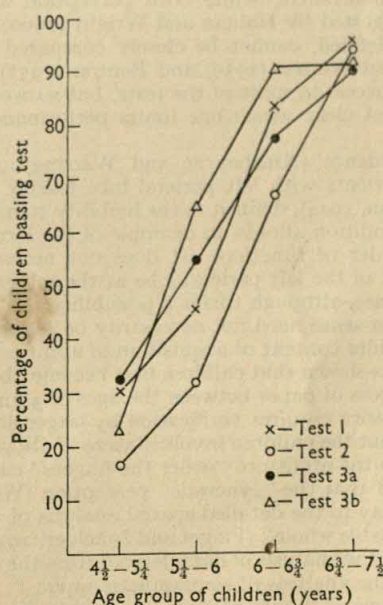
Test	Age Groups			
	4½-5¼	5¼-6	6-6¾	6¾-7½
1 ..	$\frac{6}{23}$	$\frac{10}{22}$	$\frac{20}{24}$	$\frac{21}{22}$
2 ..	$\frac{4}{23}$	$\frac{7}{22}$	$\frac{16}{24}$	$\frac{21}{22}$
3a ..	$\frac{8}{26}$	$\frac{17}{31}$	$\frac{17}{22}$	$\frac{9}{10}$
3b ..	$\frac{6}{26}$	$\frac{20}{31}$	$\frac{20}{22}$	$\frac{9}{10}$

Each fraction represents number of children who passed out of total number tested

The curves are such as would be expected were they representative of an aspect of maturation subject to normal variation. Fifty per cent. success was achieved between the ages of 5 and 6 years, and more than 90 per cent. success was achieved by the age of 7½.

The results of tests 3a and 3b correlated at the 5 per cent. level of significance in all four age groups. The results of tests 1 and 2 correlated at this level of significance only in age-group 6-6¾.

FIGURE 4



The relationship between chronological age and test performance.

The relation of test result to hand preference, and to particular fingers used in a given trial, is being explored in a large scale study.

DISCUSSION

The results show that the tasks depend for success upon the chronological age of the subjects, and do so in a very similar manner. Each set of test results conforms to the pattern which would be expected if it represented a unitary function developing in a population subject to normal variation. This result is most simply explained by supposing the limiting factor for performance of each to be the same ability, although the level of inter-test correlation was not sufficient to prove this.

The three tasks were designed to differ widely in respects other than their demands for differential treatment of sensory input from the fingers. The verbal skills required were minimal, and the preliminary procedure ensured that difficulty in comprehension would not be a limiting factor. Even the younger children, who failed in all tasks, understood what was required. Nor is it apparent how previous experience could have acted as a complicating factor, nor which ability involved in these test procedures might have acted as the common limiting factor other than the ability to treat differentially sensory input from the fingers. The available evidence suggests that this ability was acquired by the age of about $5\frac{1}{2}$ years in 50 per cent. of the present group of normal children. Whether the relationship to chronological age, as found, is the consequence of a more immediate relationship to mental age in a group of children in whom mental age is normally distributed, is left open by this study.

Task 1 required finger differentiation as such. Tasks 2 and 3 required not only a qualitative differentiation of the fingers, but one in terms of their spatial arrangement; finger differentiation was called for in terms of finger order. Yet all three tasks yielded similar results. This suggests that once sensory finger differentiation is achieved finger order simultaneously becomes available as a tool in discrimination. In other words, when the fingers are differentiated, this is in terms of their spatial arrangement from the start. This finding has obvious implications for our knowledge of the development of what some call the "finger scheme." It also supports the view that the same ability acts as limiting factor in each of the three tasks, and we have called this ability "finger sense."

At about the same stage in maturation differential finger movements become possible (Rey, 1952). The separation between this motor skill and its sensory counterpart is probably to some extent artificial. It seems likely that during maturation these two abilities are developed concurrently, while both perceptual and executive experience fortify each other as is suggested by Halnan and Wright (1960).

Finger sense, as here defined, cannot be closely compared to "finger localization," as tested for by Strauss and Werner (1939) and Benton (1955). "Finger sense" would appear to be essential to success in most of the tests, but so would a variable number of other abilities, and it is not clear which one limits performance at any given stage in maturation.

We have presented evidence (Kinsbourne and Warrington, 1962) that it is finger sense that is lost when patients with left parietal lobe lesions develop the symptom of "finger agnosia" (Gerstmann, 1924), defined as the inability to name the fingers, and point to named fingers. This condition affords an example of the localizing value, for cerebral cortical lesions, of a disorder of function. It does not necessarily indicate that it is particularly a development of the left parietal lobe at the relevant age that brings about the acquisition of finger sense, although this is a possibility.

The acquisition of finger sense need not necessarily be regarded as an isolated phenomenon, unrelated to the wider context of acquisition of abilities by the child. The study of some individual cases has shown that children first become able to place in a prescribed order coloured beads or pieces of paper between the ages of 4 and 5 (Piaget and Inhelder, 1948). Although this evidence requires verification by larger scale studies, and although there is reason to believe that the children involved were of above average intelligence, the relevance of this finding to the ability to "order the fingers" cannot be dismissed. It is, after all, at about this age that the "syncretic" perception (Werner, 1948) of the young child is supposed to give way to the detailed spatial analysis of what appeared previously as a "global or an unanalysable whole" (Piaget and Inhelder, 1948). It is entirely possible that at any stage facility in the analysis of data derived from the external world is matched by comparable facility in the analysis of somæsthetic input.

Evidence for this viewpoint is afforded by the study of deranged function. The concomitants of finger agnosia are right-left disorientation, dysgraphia and dyscalculia,

comprising the "Gerstmann Syndrome." The dysgraphia in this setting consists of a difficulty in the correct ordering of the letters comprising the word (Kinsbourne and Warrington, 1964) a disability which is shared by normal children under the age of 6 (Hildreth, 1932). The dyscalculia and right-left disorientation of the Gerstmann Syndrome are similarly based on a failure in distinguishing between the units of a linear series in terms of their relative spatial position.* This suggests that what is lost, in this case, and what is gained in children by the time they are about 7 years old, is not a differentiating ability specifically related to the fingers, but one which, when acquired, can be applied to a wide range of tasks, both involving the person, and in external space. It provides a new tool with which a wide variety of sensory information, both immediately experienced and recalled from the memory store, can be processed in ways previously not possible.

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BOOK REVIEWS

Verbal Learning and Verbal Behaviour. Edited by Charles N. Cofer. London. McGraw-Hill. 1961. Pp. vi + 241. 46s.

In 1959 a conference on Verbal Learning and Verbal Behaviour was sponsored by the U.S. Office of Naval Research and New York University. The book is a record of the seven papers delivered at this meeting together with the comments of a discussant on each paper. Dr. Cofer provides a short introduction and in his final commentary indicates the surprises in the conference's omissions and commissions.

In one sense this is a timely book since interest in verbal studies is increasing and present research is showing considerable diversity. But the conference came a little too early to reflect these interests. The book is not so much a signpost to the 'sixties as a consideration of what the work of the 'fifties has achieved. It tries to look forward but it finds, as ever, that it is easier to look back. And it has some important matters at which it can look.

Deese makes some useful comments on the lack of relationship between word frequency and free recall. Osgood argues forcibly against Bousfield (who is on the side of the angels with B. F. Skinner) that "meaning" is a central concept for verbal behaviour, and that the reactions of subjects to a semantic differential are not predictable from knowing their word-association hierarchies. Postman threads a sure way through the maze of interference theory; here is valuable criticism, as in the discussion on the unlearning variable in Melton's "Two-Factor Theory," and there are some challenging findings on the retention of high- and low-frequency words. Again the emphasis is on the problem of extra-experimental interference and the possibility of measuring it. Melton takes up this point in his discussion in which he sees the nonsense syllable playing considerably less part in future research. He also adds the caution that present evidence would not justify us in treating all long-term forgetting as resulting only from interference factors.

The final chapter is by Underwood on Eleanor Gibson's 1940 paper applying the concepts of generalization and differentiation to verbal learning. This paper has for so long been one of the leading statements in this field that comments upon it from Underwood do raise expectations. His paper is good criticism on detailed points, but overall it is disappointing. It has not the breadth of his "Interference and Forgetting" paper—and it is perhaps unfair to expect that standard every time—but in applying post-Hullian criticism to this early paper it seems somewhat off target. Even so Underwood fairly points out that the theory will have to be restated with more precision to meet present-day findings, and expresses his view that "such a revision would almost of necessity result in a completely new theory." It would seem that the ball has been played firmly into the Gibson court, and those of us who have long admired her work will hope that she will try to return it.

In all then this is a useful book of criticism; it is somewhat uneven and it should be pointed out that some of the papers, as with so many American contributions on verbal behaviour, are atrociously written. If only we could have a campaign to write less and to try to say more.

HARRY KAY.

The Historical Development of British Psychiatry. Volume I: Eighteenth and Nineteenth Century. By Denis Leigh. Oxford. Pergamon Press. 1961. 70s.

The first volume of Dr. Denis Leigh's trilogy is concerned with British psychiatrists of the eighteenth and early nineteenth centuries, the period running roughly from John Purcell's *A Treatise of Vapours* (1702) to John Connolly's last major work *The Treatment of the Insane without Mechanical Restraints* (1856). This was the time of emergence of psychiatry as a specialized medical study. During the previous century, the ecclesiastical and juridical account of insanity, which had equated it with religious heresy, had given way to medical ways of thinking about and treating the insane. But the most important writings of that period are by general physicians: William Gilbert, Sydenham, and Thomas Willis. It is only with the foundation of specialized hospitals in the middle of the eighteenth century, that the professional psychiatrist (or mad doctor) came into his own.

The first section of Dr. Leigh's book deals with the rise of specialism and a number of minor eighteenth-century writers. The three remaining sections are devoted to John Haslam (1764–1844), James Cowles Prichard (1786–1848) and John Connolly (1794–1866). Haslam, whose clinical observations prepared the ground for a rational nosology, has only in recent years been disinterred from the odium into which he fell with his involvement in

the mismanagement of Bethlem. Dr. Leigh compares him with his great contemporary, Philippe Pinel, whose casting off of the chains from the lunatics in the Bicêtre is seen as a dramatic gesture: "his book abounds in references to restraint and coercion, and his practice differed in few respects from that of Haslam." Yet "Haslam, a far more original and discerning writer was dismissed from his post and his memory allowed to lapse into obscurity, whilst Pinel was to go from success to success and his name to become a household word in psychiatry. Both men were leaders of psychiatric thought in their time, but for solid intellectual merit a study of Haslam's work will leave little doubt as to which of them has left more to posterity."

Dr. Leigh concentrates on the development of psychiatric thought and practice (an admirable history of the psychiatric services covering roughly the same period is already available in Kathleen Jones's *Law, Lunacy and Conscience, 1744-1845*) and his method is biographical. The book is well produced and illustrated, though its price makes it more suitable for the public library than the private student.

A. D. HARRIS.

Speech Disorders: Aphasia, Apraxia and Agnosia. By Sir Russell Brain. London Butterworths. 1961. Pp. vii + 184. 42s.

Traumatic Aphasia: A Study of Aphasia in War Wounds of the Brain. By W. Ritchie Russell and M. L. E. Espir. Oxford Neurological Monographs. Oxford University Press. 1961. Pp. vii + 178. 38s.

Lord Brain's short book is an expansion of the section he wrote on aphasia, agnosia and apraxia for a well-known textbook of neurology and includes also the substance of his Hughlings Jackson lecture delivered last year. It provides an admirably concise account of its subject, due regard being paid to historical, clinical and theoretical aspects, and to the need to base explanation of speech disorders on a fuller understanding of normal speech. In this connection, the author shows himself well aware of recent advances in the study of language and is favourably disposed to the analysis of aphasia from the standpoint of communications theory. Apraxia and agnosia receive more summary treatment. Although advancing no new theory, this book should be of great value to psychologists and others seeking an account of aphasia more up to date—and less wordy—than that of Henry Head.

Dr. Ritchie Russell and Dr. Michael Espir report a most useful analysis of 280 cases of aphasia in the records of the Military Hospital for Head Injuries at Oxford. Although primarily concerned with the issue of localization (for which their material was exceptionally suitable), the authors give much valuable data on the psychological features and course of traumatic aphasia, illustrated by a large number of well-presented case summaries. While the authors are rightly cautious regarding the localization of psychological function, their work leaves no doubt as to the occurrence of highly circumscribed defects in language obviously related to the locus of the injury. Some interesting cases with specific defect of memory are also included. Although the statistically-minded may cavil at the lack of significance tests (which on occasion might have been applied with advantage), this study should be read with great care by all interested in the psychological sequelae of brain injury.

O. L. ZANGWILL.

Stereotypy of Imagery and Belief as an Ego Defence. By Rosemary Gordon. British Journal of Psychology Monograph Supplement No. 34. London. Cambridge University Press. 1962. Pp. 96. 25s.

In the first half of this monograph Dr. Gordon discusses the general nature of belief; and in the second she describes a series of experiments linking political prejudices with imagery. Although the discussion is clear and enlightening, the chief interest of the monograph lies in an empirical discovery: that people who can control their visual imagery are unlikely to have stereotyped political beliefs, whereas those who cannot are as likely to have stereotyped beliefs as not. Both loss of control over imagery and stereotyping of beliefs are seen as effects of the same ego defence mechanism. This, in its turn, is held to be activated by unresolved Oedipal conflicts, its effects spreading through the various mental functions and subduing first those least open to conscious control.

There may be doubts about this kind of interpretation, but the importance of the empirical relation itself will be obvious to everyone—everyone, that is, whose curiosity extends beyond visual illusions and the T-maze. Unfortunately, though, the monograph has grave experimental weaknesses. Imagery is notoriously elusive, yet the present data come from questionnaires given out during L.S.E. and W.E.A. psychology classes! In

addition, the crucial statistic seems faulty. (In the experiment relating imagery to prejudice, $n = 22$. X^2 is calculated, neglecting both Yates's Correction and the expected frequencies below 5; and as a result P is given as <0.05 instead of >0.1 .) So the discovery may easily be an artefact, either of the group testing method or of inadequate sampling. It is sad to find theoretical and experimental sensitivities so unrelated. L. HUDSON.

Contemporary Research in Personality. Edited by Irwin G. Sarason. The University Series in Psychology, Editor D. C. McClelland. Van Nostrand. 1962. Pp. xii + 411. 54s. 6d.

This volume brings together some 40 papers published in the American psychological literature over the past 20 years. Its aim, according to the Editor, is "to provide the student with samples of empirical and theoretical issues, problems and methods characteristic of much current research in personality." This the book undoubtedly achieves but it is debatable whether it achieves anything more. O. L. ZANGWILL.

Henderson and Gillespie's Textbook of Psychiatry. Ninth Edition revised by Sir David Henderson and Ivor R. C. Batchelor. London. Oxford University Press. 1962. Pp. xii + 578. 42s.

This edition of "Henderson and Gillespie" has been extensively revised to take account of recent developments in psychiatric practice. It includes a new chapter on the relations of psychiatry to general medicine and a most useful summary and appraisal of the new Mental Health Acts. Many of the sections on specific disorders have been rewritten and some of the earlier case histories omitted. Otherwise, the book retains its original broad and humane outlook and holds the balance nicely between the biological and social aspects of mental illness. It almost certainly remains the most useful text for experimental psychologists and others seeking a clear and non-partisan account of the field of clinical psychiatry. O. L. ZANGWILL.

NEW PSYCHOLOGICAL PAPERBACKS

The Psychology of Study. By C. A. Mace. Penguin Books Ltd. 1962. 3s. 6d. A revised version of the influential little book first published in 1932.

The Psychology of Perception. By M. D. Vernon. Penguin Books Ltd. 1962. 5s. A new book in the scholarly tradition of Professor Vernon's earlier studies of visual perception.

Explorations in Personality. By H. A. Murray and others. New York, Science Editions Inc. 1962. 19s. A reprint of the well-known inquiry undertaken at the Harvard Psychological Clinic and first published in 1938.

A Study of Thinking. By J. S. Bruner, J. J. Goodnow and G. A. Austin. New York. Science Editions Inc. 1962. 15s. A noteworthy experimental study of conceptual thinking first published in 1956.

Measuring Human Motivation. Selected readings. Edited by R. C. Birney and R. C. Teevan. Insight Books. D. Van Nostrand Co. Inc. 1962. \$1.95.

Emotion: Bodily Change. Selected Readings. Edited by D. K. Candland. Insight Books. D. Van Nostrand Co. Inc. 1962. \$1.95. Selections from, among others, William James, P. T. Young, J. B. Watson, D. O. Hebb, J. V. Brady and H. Schlosberg.

Towards a Psychology of Being. By Abraham H. Maslow. Insight Books. D. Van Nostrand. 1962. 15s.

Allgemeine Psychologie. By Theodor Erismann. 1962. Berlin. Gruyter. Band III: Experimentelle Psychologie und ihre Grundlagen. 1. Teil (Band 833). DM 3.60. Band IV: 2. Teil (Band 834/834a). DM 5.80.

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Part 3

AN AFTER-EFFECT IN HAPTIC SPACE PERCEPTION

BY

JAMES J. GIBSON and FREDRIK A. BACKLUND

From Cornell University and Uppsala University

After-effects in haptic perception have been little studied. But they seem to be prominent, and easy to elicit. An after-effect of the perception of the vergence of two surfaces is described, and is shown to be independent of the slant of the surfaces. It is not "figural." Spatial perception with the hands, as well as with the eyes, is apparently based on a fluid and adaptable receptive system.

Köhler and Dinnerstein (1947) described a novel after-effect of prolonged kinaesthetic perception. The kind of perception studied is as interesting as the after-effect, for it is a matter of everyday experience but its explanation is still a puzzle. It is the ability to feel the amount of separation between the thumb and fingers of the hand. One can register this extent when blindfolded. By this means one can measure the width of an object grasped in the hand, or one can feel the span of the space between two surfaces, or one can simply feel the posture of the thumb and fingers without touching anything. This perception of separation is possible for any one of the fingers opposed to the thumb, or for all of them. It can also be equated in the two hands, which is puzzling, and it can be easily compared with an intangible but visible extent, which is even more puzzling. This ability to measure space with opposable members of the body has been called a "mandible principle" (Katz and MacLeod, 1949).

The sensitivity underlying this perception is named "feeling" in ordinary speech. It is allied to the supposed senses of touch and kinaesthesia. Its most essential single basis is probably the ability of the receptors lining the joints to register the motions and also the stable angular positions of the bones. It will here be called haptic sensitivity, to use a non-committal term.

Köhler and Dinnerstein (1947) established, in a series of experiments, that the prolonged feeling of a certain fixed extent has the effect of *increasing* the apparent separation of a larger extent and of *decreasing* the apparent separation of a smaller extent. The apparent increase or decrease was usually measured by requiring the subject to feel the edges of a bar of greater or lesser width, matching this experience with that of a variable width presented to the other hand. The range of widths used in these experiments varied from $\frac{1}{2}$ in. to about 5 in. The illusion was classified as a figural after-effect because of the analogy with enlarged or diminished visual separations which had been previously discovered by Köhler and Wallach (1944). The prolonged feeling of the fixed extent was called a period of satiation, again by analogy with the visual experiments.

Köhler and Dinnerstein (1947, p. 218) mention that if one has the subject feel a bar whose edges are not parallel but instead converge, a pair of parallel edges will thereafter seem to diverge. This seems to be an after-effect on the perception of parallelism, not of separation, and to concern a variable of shape, not of size. The authors did not

pursue this phenomenon, but it suggests the existence of other phenomenal dimensions of haptic sensitivity, and the possibility that they too, may show after-effects. One of the present authors had previously noted instances of such effects.

Haptic space perception is rich in discriminable qualities and quantities, although most of them have not been studied. The perceptual capacities of the blind are an illustration (Revesz, 1950). The ordinary person can detect the solid geometry of his surroundings in darkness, and a subject can feel the layout of a complex array of edges and surfaces with his hands alone (Gibson, 1962). Consider separation. It can be registered by a single hand up to a finger span of 8 or 9 in., but with two hands it can be registered up to an arm span of 5 ft. Although small objects are usually held between the fingers and large objects between the hands, an intermediate size judgement seems to be just as easy when a pair of fingers as when a pair of hands is used. A fixed extent can be judged between two feet as well as between two hands. The shape of an object can be successfully explored either with two hands simultaneously or with one hand successively. The curvature of an edge, or of a surface, can be detected either by running the fingers along it or by laying the palm of the hand on it. The tilt or slant of an edge or a surface with respect to the earth or with respect to the body can be perceived by feeling it actively or simply by placing the hand on it. The plane of the palm of the hand is a vivid spatial experience. Moreover seemingly complex combinations of these variables of solid geometry can be distinguished as readily as the variables themselves.

We chose to study the variable of departure from parallelism of the opposite faces of an object held between the hands. This property might be termed the *vergence* of the two planes, that is, the variable of convergence, parallelity, or divergence. Any two surfaces or edges have this property, and it is fundamental in geometry. The question was whether, after the prolonged feeling of divergent surfaces, parallel surfaces would feel convergent, and after prolonged convergent perception they would feel divergent.

It should be noted that the separation of two surfaces is a variable that goes from a minimum to a maximum whereas the vergence of two surfaces is a variable that goes from a maximum through zero to another maximum. The former may be called an *intensive* dimension and the latter an *oppositive* dimension; the senior author has suggested that an *oppositive* dimension provides the opportunity for an after-effect properly called *negative* (Gibson, 1937). The outcome can be described as a tendency for a prolonged non-neutral state of the environment to have its stimulating capacity shifted in the direction of the neutral experience, that is, for a shift of the psychophysical correspondence between stimuli and perception to occur such that a temporarily persisting stimulus value becomes more nearly equivalent to the permanently neutral stimulus value. This tendency might be called one of *normalization* in perception, and there are many examples of it among the various phenomena of sensory or perceptual adaptation. But it is admittedly not applicable to a purely intensive dimension of stimulation and experience where a normal intensity is not permanently or temporarily established somewhere along the dimension.

APPARATUS AND PROCEDURE

The apparatus consisted of two surfaces on which the palms of the two hands could be placed. The palm-boards could be adjusted to parallelism or to any angle of divergence or convergence up to 90°, and the angle of the planes could be read by a protractor with pointer which was accurate to a fraction of one degree. The boards were 12 cm. wide, 21 cm. long, and they were 14 cm. apart at the central axis of each.

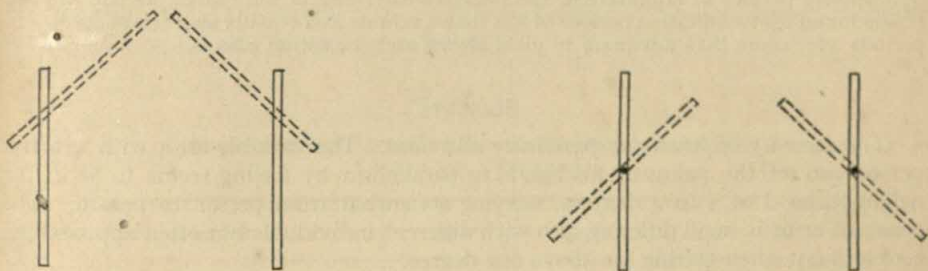
In the simplest arrangement, the subject sat squarely at a table on which the apparatus lay, put his arms behind a cloth curtain and pressed his hands gently on the two upright

palmboards, which were straight in front of him. He was never allowed to see the apparatus, but he was not blindfolded. He was allowed to move his wrists so as to make the surfaces diverge and converge and then set them to parallelism. This adjustment could be made with confidence. Several repetitions of the setting were then required so as to establish the objective equivalent of his feeling of parallelism. Next, the surfaces were set by the experimenter at some degree of inclination or "vergence" and the subject was required to keep the palms of his hands in contact with them for some period of time. Finally, he was again asked to set the palmboards to parallel, moving them until he was satisfied, and the protractor was read by the experimenter.

In general, the result of this procedure was that the new setting deviated from the objective parallel in the direction previously felt. After feeling convergent surfaces, for example, parallel surfaces felt divergent and had to be made somewhat convergent in order to be felt parallel. In some hundreds of trials with dozens of subjects there was no exception to this result, and a discrepancy of 10° between the two settings was not unusual.

This result seemed to imply a tendency for the phenomenal norm in this kind of haptic perception to shift along its dimension, that is, for a persistently abnormal physical degree

FIGURE 1



The palmboards of the first and second type, viewed from above. With the first apparatus, the movement of the hands in adjusting to parallelity included a fore-and-aft component whereas in the second the movement was wholly one of rotation of each palm on an axis close to the knuckles. The former seems to yield an extra feeling of opposition or balanced forces when the hands are parallel; the latter only a feeling of the spatial planes of the palms. Hence the latter is preferred for the study of spatial perception based on pure sensitivity of the joints.

of vergence to be felt as more nearly normal or parallel than it is. But, with the arrangement described, this conclusion could not be drawn. Each of the two surfaces might have been perceived as having an absolute slant from the sagittal plane of the body or from the reference plane of the table and chair and room, all of which were coincident. Each hand had an independent feeling of slant besides both hands having a feeling of vergence. Consequently the outcome might have been due to a normalization of each hand with respect to the framework of haptic space instead of a single normalization with respect to the higher-order quality of vergence. The two variables would have to be isolated experimentally.

A series of experiments was undertaken in order to determine the accuracy of the perception of vergence with the hands, to measure the after-effect, that is, the shift in the feeling of parallelism caused by a period of fixed vergence, to measure this after-effect as a function of the amount of vergence during the period, to measure the after-effect as a function of the duration of this period and determine how rapidly it dissipates after the period, and finally to separate the effect of haptic slant (deviation of the hand from a reference plane) from the effect of haptic vergence (deviation of two hands from parallel).

Two types of apparatus. The first pair of palmboards constructed had a protractor fixed to one and a pointer fixed to the other so that the vergence, positive or negative, could be read directly by the experimenter. The two boards had a common axis centred half way between them so that their midlines were 14 cm. apart when parallel but were a little less when divergent or convergent. The movement in adjusting each could be felt as slightly orbital rotation instead of pure rotation. After the first subjects were tested, a new apparatus was built, each palmboard having its own axis of rotation on the midline of the surface touched by the palm of the hand. (Fig. 1.) These axes were 14 cm. apart. This

device required two protractors each with its own pointer so that the vergence could be obtained only by algebraic addition of the two pointer-readings, positive or negative, from zero. The slant of each palm, however, could be recorded as a deviation from the sagittal plane. This apparatus was mounted on a circular base which could be placed on a circular table. When the subject was seated on a circular stool askew to the wall of the room, only the feeling of the axis planes of the body and the sight of the flat screen in front of the eyes could anchor his haptic frame of reference.

Method of measuring the after-effect. In these experiments each measurement used for computation was obtained from two trials one with a convergent and one with an equally divergent period of preceding adaptation. The after-effect was taken to be half the sum of the two deviations from parallelity, convergent or divergent. The difference between the two adjustments, as computed from the pointer-readings, was always in the expected direction. Each pair of settings thus gave its own control. This method yields a very conservative measure of the after-effect, but it is unchallengeable and it prevents the building up of an illusion, that is, a possible cumulative constant error in the perception of parallelity (or straightforwardness). This is similar to the phenomenon Köhler and Wallach (1944) have called "permanent satiation." Even with the successive cancellation of effects by opposite periods of stimulation, the negative after-effects were large, as will appear. It was found that adaptation periods of less than a minute and equally short rest or recovery periods were more than adequate to yield strong and consistent effects.

RESULTS

The accuracy of perceiving parallelity and slant. The variable error with which a person can set the palms of his hands to parallelism by feeling seems to be in the neighbourhood of 3 to 4 degrees, varying somewhat from person to person. The constant error is small differing also with different individuals but often approaching zero and not often getting far above one degree.

For one set of 20 adjustments for each of 12 subjects using the apparatus first built, the S.D. was 3.11° and the C.E. was 0.83° . For another set of 20 adjustments for each of 12 new subjects using the second apparatus, the S.D. was 3.41° and the C.E. was 1.55° .

Many informal measurements using one hand have been made in connection with experiments on the perception of surface slant. The accuracy with which a palmboard can be adjusted to the sagittal plane of a subject's body, or the frontal plane, or the horizontal plane, is in all cases comparable to the figures reported above.

This acuity for the geometrical properties of manually experienced space is surprising when one considers its basis in sensitivity. The relative angular positions of the bones of the two hands, or the absolute angular posture of the bones of one hand, can only be registered by a combination of patterned inputs from the linings of the joints. Since the pioneer work of Goldscheider (1898), it has been recognized that the degree of contraction of muscles can make little or no contribution to this kind of postural perception, and that it must be a matter of articular sensitivity. In order to feel two hands as accurately parallel, however, a large number of joints (at least six) have to co-operate in providing a high-order pattern of relations among the inputs from these joints. With such an intricate receptive system it would not be surprising to find that it was susceptible to a constant error after a period during which the pattern was asymmetrical and during which no movements of the joints occurred to anchor the limits of the input.

The amount of the negative after-effect. The extent to which parallel palmboards feel divergent after holding them convergent (and convergent after holding them divergent) is so great that no individual yet tested has failed to show the effect. In order to make them feel parallel the surfaces usually have to be set from 5° to 10° off parallel in the direction previously experienced. This is when the angle previously experienced is between 20° and 90° .

One of several experiments will be reported which measured the after-effect under various conditions. The improved apparatus was used, and also the method of opposite trial-pairs for obtaining a measure of the effect. Short periods for adaptation and recovery were employed. The palmboards were set by the experimenter at a certain angle of vergence (22.5° , 45° , or 90°) and the subject was asked to hold his hands against them for 20 sec. He next had to adjust them so they felt parallel (5 to 15 sec.) and then was allowed a rest period for the remainder of one minute (25 to 35 sec.). The opposite angle was immediately presented and another adjustment was made. Half the difference between the two readings was taken to be the after-effect of the pair of trials. A measure was thus obtained every two minutes. Ten such measures were obtained for each of the three angles presented. The order in which these were presented was counterbalanced among different subjects. Twelve subjects who had no knowledge of the expected outcome were used.

The results are given in Table I. Each mean and SD is based on 120 measures. It can be noted that the greater is the angle of inclination of the surfaces felt, the more is the effect on the perception of parallelism.

TABLE I
NEGATIVE AFTER-EFFECTS RESULTING FROM DIFFERENT
ANGLES OF VERGENCE

		22.5°	45°	90°
Mean		6.08°	10.29°	11.04°
S.D.		4.01°	5.60°	6.43°

The after-effect as a function of vergence. Another experiment was performed to measure the effect with six different angles of vergence instead of three. The procedure was the same except that the presentation of these angles was randomized for each subject, and the time allowance for a trial was reduced to 43.5 sec. instead of a minute. This was accomplished by using a set of timers which gave signals to indicate the beginning of the adaptation period (18 sec.), the signal to adjust (8 sec. allowed, plus another 3.5 sec for recording), the beginning of a rest period (14 sec.) and then the beginning of a new adaptation period. The subjects were instructed in the use of these signals. Each subject made three opposite pairs of adjustments for each of six angles of vergence without interruption. Twelve naive subjects were used.

The results are given in Table II. There is an increasing effect with the amount of convergence or divergence of the hands. At 11° , which is about three or four times the just noticeable angle of vergence, the after-effect is small.

TABLE II
AFTER-EFFECT AS A FUNCTION OF THE ANGLE OF VERGENCE

	11.25°	22.5°	37.5°	45°	67.5°	90°
Mean	2.50°	3.25°	4.79°	5.11°	7.60°	6.67°
S.D.	3.24°	2.26°	3.81°	4.50°	4.84°	4.39°

Duration of the adaptation period. The adaptation seems to build up rapidly. How short a period will still yield some after-effect, and does the process continue to build up during longer periods of adaptation? The timer used in the preceding experiment for signalling a period of 18 sec. could be set for any other duration. Five durations were investigated, from 12 sec. to one minute. The interval for adjustment and recording was kept at 11.5 sec. and the rest period remained at 14 sec. but the adaptation period was randomly varied. Each subject made four pairs of adjustments after each of the five durations. Eighteen undergraduates served as subjects.

The results are given in Table III. The after-effect is equally great at all durations. Seemingly whenever the hands are kept motionless in such a "holding" position

TABLE III
AFTER-EFFECT AS A FUNCTION OF ADAPTATION TIME

	12 sec.	18 sec.	27 sec.	40 sec.	60 sec.
Mean	9.79°	8.82°	8.84°	8.82°	8.99°
S.D.	4.36°	4.21°	4.88°	4.71°	5.23°

adaptation occurs and promptly reaches its maximum. In this type of haptic experience it does not show any further increase with time. There is no indication of the cumulative effect of satiation that Köhler and Dinnerstein (1947) found for the after-effect of feeling a certain width.

Duration of the recovery period. If adaptation to vergence of the hands occurs promptly does it also dissipate promptly when the hands are no longer kept in the vergent posture? In this and the previous experiment, the subject was asked to place his hands, palms down, on the table top during the rest period. The timer which signalled a rest period of 14 sec. could be set to give other durations. Five durations were investigated from zero to 56 sec. The adaptation period was set at 18 sec. and the interval for adjustment and recording remained 11.5 sec., as before. Each trial was preceded by a trial with an opposite angle of vergence. Twelve subjects were run through the same procedure as in the previous experiment.

The result are given in Table IV. There is no difference between the after-effect obtained after a 7 sec. recovery period and that obtained after a 56 sec. recovery period. There is no tendency for the preceding effect to cancel out the succeeding effect when seven or more seconds of rest are allowed. With no rest period a decrease in the after-effect appears, the left-hand column showing the smallest mean and the largest standard deviation. The preceding after-effect does seem to cancel the succeeding effect in some degree when no rest period is permitted.

TABLE IV
AFTER-EFFECT AS A FUNCTION OF RECOVERY PERIOD FROM THE
PREVIOUS OPPOSITE TRIAL

	0 sec.	7 sec.	14 sec.	28 sec.	56 sec.
Mean	4.43°	6.18°	5.97°	5.81°	6.12°
S.D.	5.74°	4.29°	3.34°	4.58°	5.52°

Comparison of convergence and divergence. With our palmboards it is easier to converge the hands through a given angle (fingertips together) than it is to diverge them through the same angle (fingertips apart). There is more muscular strain in divergence, although the joint of each wrist permits a wide angular rotation. Conceivably the degree of muscular strain might influence the tendency to adaptation. Since the after-effects produced by divergence and by convergence were separately recorded (although opposite pairs were combined to yield a single measure) it is possible to compute separately the mean after-effect of divergent and of convergent adaptation. If the data reported in Table I are split up and recomputed, the results are those given in Table V.

TABLE V
AFTER-EFFECT WITH CONVERGENCE AND WITH DIVERGENCE

22.5°		45°		90°	
Conv.	Div.	Conv.	Div.	Conv.	Div.
3.67°	3.38°	4.77°	6.39°	7.37°	5.73°

The effect of holding the fingers outward is not consistently different from that of holding them inward. This result is confirmed by the data of another experiment not reported. Presumably muscular strain does not influence the after-effect. It is probably a phenomenon based on the sensitivity of the joints.

THE EXPERIMENTAL SEPARATION OF THE BIMANUAL AND THE UNIMANUAL EFFECTS

We have been speaking of adaptation to vergence as if a pair of hands constituted a single sense organ. But it is possible that all the effects so far considered could be explained as an after-effect in each hand separately with respect to the sagittal plane of the body, that is, adaptations to the feeling of slant in each hand. The first question is whether such a unimanual adaptation does in fact occur.

The after-effect of slant from the fore and aft plane. A subject could be seated squarely in front of the screen, as before, with one hand placed on one of the palmboards and the other hand resting. He could turn the palmboard from right to left until it seemed to be in the fore-and-aft plane, and the protractor could be read by the experimenter. In the present experiment, six of these preliminary adjustments were made by each hand and the mean of these readings was taken as the normal plane from which an opposite pair of 45° slants would be presented. The subject was given one-minute trials consisting of 20 sec. of adaptation, a period for adjustment to the apparent normal plane, and about 30 sec. of rest. Rightward and leftward slants were given in each pair of trials, and the after-effect was taken as half the difference between the two settings. Ten measures of this after-effect of slant were obtained for each hand. Six subjects were used.

For comparison, a third condition was introduced in which the subject held *both* hands on the palmboards at 45° slants inward or outward, which is equivalent to a 90° convergence or divergence. The after-effect of this vergence was measured in the same way and with the same time intervals. The three experimental conditions, left hand alone, right hand alone, and both hands together, were counter-balanced among the six subjects.

The results are given in Table VI. Evidently a negative after-effect, or normalization, does occur in each hand separately, and this is consistent with what is known about the perception of slant in general. Moreover if one adds the after-effect in one hand to that in the other hand the sum is about the same as the after-effect for vergence obtained with two hands. This suggests that the bimanual effect can be accounted for completely by the unimanual effects.

TABLE VI
UNIMANUAL AND BIMANUAL AFTER-EFFECTS

	<i>Left-hand</i>	<i>Right-hand</i>	<i>Both hands</i>
Mean	3.06°	6.00°	8.80°
S.D.	3.13°	3.46°	8.72°

Before reaching this conclusion, however, another experiment should be performed. It is possible to test the effect of vergence alone, eliminating any possible influence of the effect of slant.

The after-effect of vergence without slant. When the arms are held straight forward one cannot make the hands converge or diverge without also making the palms slant with respect to the sagittal plane of haptic space. But if one holds the arms to the left (or right) it is possible to make one palm coincide with the sagittal plane and the other with the frontal plane, separated by 90°. If one shifts from 90° convergence to divergence the hands exchange positions but still coincide with the normal planes. Utilizing this possibility, a comparison was made between the after-effect obtained when the arms were held in one of these sideways positions and that obtained in the forward position.

For this experiment the apparatus was fixed in one of three positions on the table, to subject's left, to his right, or centred as in previous experiments. When the off-centred palmboards were prepared for a trial, one of them was always parallel to the

TABLE VII
THE AFTER-EFFECT OF VERGENCE WITH AND WITHOUT
ACCOMPANYING SLANT

	<i>Left</i>	<i>Right</i>	<i>Centred</i>
Mean	8.67°	7.96°	6.93°
S.D.	2.67°	3.34°	2.54°

subject's shoulders and the other was perpendicular to this axis. The after-effect was measured under the conditions applying to Table I and the last column of Table VI. Three measures of the after-effect were obtained from each subject for each of the three experimental conditions. There were six subjects.

The results are given in Table VII. A pronounced after-effect appears with the two offcentred conditions. It is certainly not less in amount than that with the

familiar condition heretofore studied. This bimanual after-effect can *not* be accounted for by adaptation with respect to a normal plane. The implication is that the feeling of parallelism of two hands is independent of the feeling of slant in either hand, for it can be experimentally isolated.

The results of the last two experiments may seem to be contradictory, but they are not necessarily so. The haptic sensory system, like others probably, is not based on a single sense organ but on multiple organs. The two hands can function as a unit, or one hand as a unit (or one finger as a unit) without interference. The higher order unit can register information of higher order, or at least do so more easily, and we should feel no compulsion to explain the higher in terms of the lower. Perhaps the different after-effects we have studied occur at different levels of the haptic system at the same time. The after-effect with respect to parallelism and that with respect to the framework of space do not exclude one another.

DISCUSSION

The explanation of after-effects in haptic, or "tactual-kinaesthetic" perception is not immediately evident. Of those measured, there is an after-effect of perceiving *extent*, an after-effect of perceiving *slant*, and an after-effect of perceiving *non-parallelism*. There are probably many others, some being known or suspected. A theory might usefully wait until this mode of perception is better understood.

On the assumption, however, that its most essential single basis is the sensitivity of the joints, a few suggestions are possible. Bodily posture is continually changing during waking life, for purposes of locomotion, manipulation, and tactual exploration, and this means that the joints are continually rotating. Their receptive surfaces register the amount and direction of a rotation and also the stationary angular position of the joint. But it is conceivable that the position-registration may "drift" when no rotations have occurred for some period of time. This would be the more likely if the position was biased or eccentric in some fashion. The after-effects seem to follow from a period of non-rotation of the joints. They consist of a constant error in spatial perceptions based on joint position.

The fact of angular position-registration by the joints of the human body was demonstrated by Goldscheider (1898). Elaborating this fact, one notes that the joints of the shoulder, elbow, wrist, and hand compose a hierarchy of hinges, and that the corresponding angles thus form a sort of branching vector-space for each limb, with a superordinate space for the whole skeleton. In this way the experience of the spatial arrangement of all bodily members may be given, together with whatever they touch.

The way in which the inputs from a hierarchy of joints are integrated or combined is not known. The equivalence of seemingly different combinations is a puzzle. Nevertheless there must be lawful invariant relations among them. Haptic space perception, and the anomalies or illusions of this perception which we have just begun to study, probably depend on these invariants. Constant errors in the discrimination of extent, slant, and vergence prove at least that a flexible neural system is involved. Terms like "adaptation," "satiation," "normalization," or "habituation" are suggestive of what may be going on, but it is too soon to decide between them.

These experiments were mostly performed at Cornell in 1960. The junior author has continued the research after his return to Uppsala. The work was supported, in part, by Contract NONR 401(14) between Cornell University and the Office of Naval Research. This paper was written in honour of Professor Wolfgang Köhler but too late for inclusion in his *Festschrift*.

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REpetition AND CONFIRMATION OF MESSAGES RECEIVED BY EAR AND BY EYE

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A rating method was used to obtain operating characteristics for 60 heterogeneous words. A single message was heard in noise, or seen briefly in a tachistoscope. It was repeated until it had been assigned to the highest accuracy category ("confirmed") or sent a maximum of six trials. The comparisons showed that it matters little whether reception is by eye or by ear. Whether within a trial or over successive repetitions, accuracy of reception is a direct function of the confidence rating and is relatively independent of the intelligibility level. Neither do the accuracy of reception or the distributions of rating categories change markedly over trials. Although no direct test was made, it appears that accuracy of reception is not lessened by the task of rating. Both visual and auditory data are fitted reasonably well by predictions made from a simple stochastic model based on the assumptions that (1) intelligibility, (2) probability of a correct acceptance, and (3) probability of an incorrect acceptance remain constant over successive repetitions. The model fits the visual better than the auditory data, as might be expected, since conditions of reception are more homogeneous over trials for vision than for audition.

INTRODUCTION

It is of considerable interest to explore the relationship between vision and audition especially where responses to complex patterns are involved. There is evidence for processing in the nervous system at some stage later than, and common to, the eye or ear. The conversion of audible into visual patterns is, of course, the basis of the sound spectrograph (Potter, 1945) and the inverse conversion is the basis of the Pattern Playback used for research in the perception of speech (Cooper, Liberman and Borst, 1951). Miller and Heise (1951) concluded that a subject's threshold for integrating a variable tone into a pattern is approximately what one would expect for vision if frequency and time were transformed into vertical and horizontal distances. It has been shown recently by Hirsh and Sherrick (1961) that the time separation required for 75 per cent. correct judgement of perceived temporal order is about 20 millisecon. and is independent of the sense modality employed, at least for audition, vision and vibration. The authors feel that "it seems to point some kind of time organizing system that is both independent of and central to the auditory mechanisms." Kinney (1961) measured discrimination for analogously patterned stimuli presented to the ear and to the eye. Frequency was made analogous to vertical separation, and time to horizontal separation of visual stimuli. Similar discrimination functions were found for the two senses, including improved discrimination with one repetition of the basic elements but no further increase with further repetitions.

Recently, we compared the receiver operating characteristic (ROC) curves for auditory and visual reception (1962). Spoken messages were received by earphones in a wide band noise at three different speech-to-noise ratios and the visual messages were exposed briefly in a tachistoscope for various durations so as to give intelligibility levels corresponding to those of the auditory case. For both auditory and visual presentation it was found that the accuracy of reception is (1) directly related to the confidence rating, (2) relatively invariant over a range of signal-to-noise ratios, (3) is

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not impaired by the task of rating. Pollack and Decker (1958) had already found this to be so for audition and furthermore showed that (4) the accuracy of reception, and (5) the distributions of rating categories changed very little with successive repetition.

The present study was designed to answer the following general question: How do the auditory and visual modes of reception compare over successive repetitions of a message? The specific comparisons were with respect to

- (1) intelligibility;
- (2) the relation between intelligibility and confidence ratings;
- (3) the distributions of confidence ratings;
- (4) operating characteristics;
- (5) the application of a simple stochastic model, based on the assumption that intelligibility, the probability of a correct acceptance, and the probability of an incorrect acceptance remain constant over successive repetitions.

Suppose that a receiver is required to decide whether the message he has recorded is or is not the message actually received. The degree of certainty needed for confirmation—the criterion—may be controlled by instructions or by paying for correct decisions and levying costs for wrong decisions, or by other reinforcement techniques. A sequence of decisions is generated by repeatedly sending each of a set of messages until it is confirmed or has been sent a specified number of times. On any presentation (trial) of a message, the receiver records a message, and, immediately following, gives a confidence rating of his accuracy of reception on a scale of six. A rating of "one" indicates very high certainty while a rating of "two" indicates moderate certainty and so on down to "six" which indicates the lowest degree of certainty or high uncertainty. A message is repeated until a rating of one is given (arbitrarily called a confirmation), whereupon the next word in the set is sent. A maximum number of trials may be set, and if a confirmation does not occur, the sequence of trials is ended and the next word in the set is sent.

Let $p(A)$ and $p(B) = 1 - p(A)$ be the mean probabilities of a correct and incorrect reception, respectively, on a given trial. Then

$$p(A) + p(B) = 1.0 \quad \dots \quad (1)$$

Define R_j as the accuracy rating corresponding to the j th confidence level. If in a given situation the receiver assigns a message to one of the k mutually exclusive confidence levels, then

$$\sum_{j=1}^k p(R_j) = 1.0 \quad \dots \quad (2)$$

on the assumption experimentally justified by Egan, Schulman, and Greenberg (1959) that a receiver can adopt multiple criteria in making his ratings and that he performs as well as when he uses a simple binary decision procedure. A criterion of acceptance or rejection corresponding to level c would, under the binary-decision procedure, include all those messages that would have been rated less than c . Thus, the ordinate and the abscissa of the point on a graph for the criterion defined by c is given by

$$P(R_c/A) = \sum_{j=1}^c p(R_j/A), \quad \dots \quad (3)$$

where $p(R_j/A)$ is the conditional probability of a receiver putting his response in the j th category given that he recorded the message *correctly*, and

$$P(R_c/B) = \sum_{j=1}^c p(R_j/B), \quad \dots \quad (4)$$

where $p(R_j/B)$ is the conditional probability that the receiver puts his response in the j th category given that he *incorrectly* recorded the message.

The probability of an acceptance ("yes") under the criterion c , P_{yc} , is given by

$$P_{yc} = P(R_c/A)p(A) + P(R_c/B)p(B) \quad \dots \quad (5)$$

and the probability of a rejection ("no"), under the criterion c , P_{nc} , by

$$P_{nc} = \sum_{j=c+1}^k p(R_j/A)p(A) + p(R_j/B)p(B) \quad \dots \quad (6)$$

$$= 1 - P_{yc}$$

Henceforth in using Equations (5) and (6) the subscript c will be omitted, for it should be clear that the probability of acceptance or rejection depends on c . The pairs of sums defined by Equations (3) and (4) for $j = 1, 2, \dots, c, \dots, k$ generate the points of the receiver's operating characteristic (ROC), that is, the curve which shows how the rate of correct acceptances changes with the rate of incorrect acceptances ("false alarm rate"). Theoretically, the number of category ratings allowed, k , is unlimited. How many are possible, and what is optimal is a question of fact yet to be answered.

Clearly,

$$P_y + P_n = 1.0 \quad \dots \quad (7)$$

We define two terms: p_{cc} , the probability of a correct confirmation, and p_{ic} , the probability of an incorrect confirmation which are, in the order given, the two terms of Equation (8). Thus

$$P_y = P(R_c/A)p(A) + P(R_c/B)p(B) = p_{cc} + p_{ic} \quad \dots \quad (8)$$

under the assumptions (1) that p_{cc} and p_{ic} remain constant over successive repetitions and (2) that the probability of a correct reception of the i th message on any trial is equal to $p(A)$ for all $i = 1, 2, \dots, N$, i.e. that $p(A)$, $p(R_c/A)$ and $p(R_c/B)$ are constant over trials. This intuitively improbable assumption of a trialwise amnesic observer will be discussed later.

These assumptions lead to a simple stochastic model which gives some non-trivial predictions. Let N be the number of messages, not necessarily all different, in a set, and let each of them be sent over and over to a receiver, as described above. It can be shown that (1) the expected number of messages correctly confirmed on the n th trial, $T_{cc}(n)$ and (2) the expected total number confirmed *after* the n th trial, $T_y(n)$, is

$$T_{cc}(n) = Np_{cc}(1 - p_y)^{n-1} \quad \dots \quad (9)$$

and

$$T_y(n) = N \left(\frac{p_{cc}}{p_y} \right) [1 - (1 - p_y)^n] \quad \dots \quad (10)$$

Finally, the expected total number of messages that have occurred up to and including the n th presentation, $T(n)$, is given by

$$T(n) = \left(\frac{N}{p_y} \right) [1 - (1 - p_y)^n] \quad \dots \quad (11)$$

Several experimental tests of this model by Egan, Clarke and Carterette (1956) and Carterette (1958) show it to be a good engineering approximation in the computation of the various probabilities associated with the transmission and confirmation of messages under the conditions specified. Relatively homogeneous messages (50 spondees) were used in these experiments. If subsets of high homogeneity are considered separately in computing theoretical curves, the data fits the theoretical curves even better. Details of the above statement may be found in several papers, especially one by Egan (1957).

The rating method for obtaining operating characteristics has been used here as being essentially equivalent to, and more economical than, the method of binary decision message acceptance or rejection. At least this is so in dealing with recognition of such complex material as words. Recently Egan, Schulman, and Greenberg (1959) have shown, for the case of detection of a pure tone in Gaussian noise, that a (trained) receiver performs as well "when he adopts the multiple criteria necessary for the rating method as when he adopts the single criterion required by the binary-decision procedure."

The model of the operating characteristic used in this paper is an imprecise analogue of the one first described by Peterson, Birdsall, and Fox (1954) and elaborated by Tanner and Swets (1954), Tanner, Swets and Green (1955), Swets, Tanner and Birdsall (1955), and Tanner (1956). A broad review of detection theory and psychophysics has been given by Swets (1961).

Peterson, Birdsall and Fox say, "Suppose an observer is given a voltage varying with time during a prescribed observation interval and is asked to decide whether its source is noise or is signal plus noise. What method should the observer use to make this decision and what receiver is a realization of that method?" Whatever optimum method of decision is chosen the optimum receiver will be one whose output is either the value of the likelihood ratio of the input voltage over the observation interval, or some monotonic function of the likelihood ratio. The optimum observer chooses an operating level and only when the receiver output exceeds this level does he conclude that both signal and noise were present at the receiver input.

For a fixed operating level and signal-to-noise ratio there are two conditional probabilities, one of detection, $P_{sn}(A)$ and one of a false alarm, $P_n(A)$. The receiver operating characteristic (ROC) is the curve generated by the set of points defining all possible operating levels for a fixed power ratio of the signal and noise. A family of ROC curves is generated by varying signal-to-noise ratio. The ROC curve is useful for evaluating receiver performance. Obviously, the receiver must choose, or be set at, an operating level and the choice must depend on the values and costs assigned to detections and false alarms, and the *a priori* probability of the signal.

Tanner and Swets (1954) used the above concepts, clearly interpreted as a theory of visual detection for the human. The observer is asked to look, at a fixed time and place, for a signal whose properties he knows. Following his observation, he is required to say whether or not the signal occurred. They suppose that the value of the receiver input upon which detection depends varies randomly whether or not the signal is present. On this assumption, asking the observer to say that the signal was or was not presented is equivalent to asking him to decide whether or not to accept the hypothesis that a signal existed during the observation interval. Three more assumptions are implied by their treatment: sensory systems (1) transmit information, (2) are noisy channels and (3) can make optimal use of the information transmitted. In short, an observer in a classical "yes-no" psychophysical experiment can set up a criterion. His decision is "yes" or "no" depending upon whether or not the observation is in the criterion. He is, in effect, a kind of computer. The basic idea was extended beyond simple signals to compound signals. (Marill, 1956; Green, 1958). Egan and others, using the model more heuristically, found that the operating characteristic was fruitful when used with such complex materials as words even though the experimentally defined quantities so far have not been interpreted theoretically. (Egan, Clarke and Carterette, 1956; Egan 1957; Carterette, 1958; Pollack and Decker, 1958; Carterette and Cole, 1962.) It should be clear, then, that the operating characteristic is here used as a shorthand for describing a receiver's rate of change of "hits" as a function of the rate of change of "misses."

METHOD

Subjects

Three subjects served in both the auditory and visual phases of the experiment. The two authors served as both experimenter and subject. A paid undergraduate was the third receiver.

Auditory apparatus

The apparatus was a high quality interphone. The talker spoke into a Stevens OD-4 Microphone system. The speech level was monitored by a Weston VU meter and passed through a Krohn-Hite Model 330-M band-pass filter set for 50-7,000 cs. The signal was then mixed electrically with the output from a General Radio Company random noise generator Type 1390-A and sent to the receiver. The receiver listened with Telephonic TD-39 earphones (300 ohm) with MC-162-A cushions.

Visual apparatus

A Gerbrands Harvard Tachistoscope using 4-watt General Electric Daylight argon tubes as a light source was mounted on a small table. The luminance of the stimulus chamber was 177.0 mL., and that of the background was 17.0 mL., as measured with a Luckiesh-Taylor brightness meter. The duration of the light flash for stimulus presentation was measured by an electronic counter.

Stimulus materials

Stimulus words were chosen from the Thorndike-Lorge word count (1944) and are listed in another paper (Carterette and Cole, 1962). Three groups of 20 words each were randomly chosen with mean frequencies of the groups being 65, 127, and 302 respectively. These means were computed from the average of the columns *T* and *L* counts in the word list. Each of the words was nine letters long. Random lists of 120 words (each stimulus word represented twice) were compiled. One such list, randomly chosen, determined the order of presentation in each session. The stimulus words were typed individually in Elite capital letters on rectangular 76 mm. by 127 mm. white cards. Each word was 20 mm. long and 2.5 mm. high, and was centred on the card.

PROCEDURE

General setting

The auditory apparatus was set up in two adjoining rooms. The microphone was mounted in a 76 × 76 × 32 cm. box made of acoustical tile, with a VU meter at its rear. The speech level was kept as nearly as possible to a mean reading of zero on the VU meter. Speech-to-noise ratios were different for different receivers since it was desired that the mean articulation scores be about the same for all receivers. The speech-to-noise ratio was changed by raising or lowering the speech level relative to a constant noise level. The average sound pressure level of the noise was held constant at 73 Db. (re 0.0002 d./cm.²). The listener was seated at a desk in the adjoining room. The signal for a stimulus presentation was a small light which flashed at 10 sec. intervals and was controlled by a rotary timer. The stimulus followed the onset of the light by one sec. The subject wrote his responses on lined 3 × 5 in. cards numbered 1-120.

The visual situation differed only slightly from the auditory one. The subject and experimenter were in the same room, and the click of the rotary timer, rather than a light, signalled the presentation of the stimulus word. The subject was dark adapted for 10 min. before the start of a session. Again, he recorded his responses on white cards placed on the table below the facepiece of the tachistoscope. A dim orange light adequate for the experimenter to work in, provided for the receiver to see the responses as he wrote them.

The stimulus words were presented on a non-reflecting black background card. A new card was inserted after each confirmation (*R*₁ response) or after six presentations of the stimulus. There was a small fluctuation in the exact location of the stimulus word on each initial presentation, but not on the repetitions of the word.

The duration of exposure of a word in a tachistoscope was used to equate the probability of correct responding in the visual setting with that in the auditory setting. The method avoided eye-movements. Projecting the letters through a screen which randomly removed areas of the letters may seem a better way of achieving an analogue to acoustic "signal-to-noise" ratio. We felt that the tachistoscope gave us better control; in any case a definition of signal-to-noise ratio that applies equally well to both our situations is not easy.

Instructions

Several days before the initial session, subjects were given the list of words, and they were asked to learn them. The criterion for learning was the ability to write 58 out of the possible 60 words upon request. The stimulus set then constitutes a known set. The influence of familiarity of the message set has been investigated elsewhere and does not enter as an experimental variable here. See Pollack and Decker (1958).

At the beginning of the experiment, each subject was told to respond as quickly as possible to each word and rate each response as to the certainty that his response corresponded to the stimulus. The instructions for the category ratings were similar to those used by Pollack and Decker (1958) (see Table I).

TABLE I

CONFIDENCE RATING SCALE FOR JUDGING THE ACCURACY OF MESSAGE RECEPTION

- 1 = Positive that I received the message *correctly* (R_1)
- 2 = Fairly certain that I received the message *correctly* (R_2)
- 3 = Uncertain, but I think that I received the message *correctly* (R_3)
- 4 = Uncertain, but I think that I received the message *incorrectly* (R_4)
- 5 = Fairly certain that I received the message *incorrectly* (R_5)
- 6 = Positive that I received the message *incorrectly* (R_6)

The subject was told that each word would be repeated until he had confirmed it (rated it R_1), or had made 6 responses to the word with confidence ratings of R_2 or greater.

RESULTS

The raw data for the experiments were the set of responses to the messages sent— for each message unit there was a response with its accuracy rating. The response was either the same as the message sent (correct) or different (incorrect). If N messages (not necessarily all different) were sent on the first trial, estimates of the various quantities were made as explained in Table II. Exactly the same procedure was followed for all successive trials, and for all experimental conditions. The data for all receivers were combined before the various proportions were computed. Only data for the first five trials are used inasmuch as receivers knew that the sixth trial was the last.

TABLE II

Rating category, R_1	A Correct	B In- correct	Auditory-High						
			Total	$p(R_1/A)$	$P(R_6/A)$	$p(R_1/B)$	$P(R_6/B)$	$p(A/R_1)$	$p(R_1)$
$j = 1$	705	3	708	0.752	0.752	0.011	0.011	0.996	0.590
$j = 2$	112	5	117	0.120	0.872	0.019	0.030	0.957	0.097
$j = 3$	74	30	104	0.079	0.951	0.114	0.144	0.712	0.087
$j = 4$	27	36	63	0.029	0.980	0.137	0.281	0.429	0.052
$j = 5$	15	77	92	0.016	0.996	0.293	0.574	0.163	0.077
$j = 6$	4	112	116	0.004	1.000	0.426	1.000	0.034	0.097
Totals ..	937	263	1,200	—	—	—	—	—	—

The raw data for the first trial only of the high intelligibility condition (H) for auditory reception are shown as an example. Computations of the various proportions were made as illustrated below. The total number of messages sent on trial 1 was 1,200 of which 937 were received correctly and 263 were received incorrectly, regardless of the rating category. On the second trial the number of messages sent was $1,200 - 708 = 492$, and the same treatment was given these data, and so on.

$$1. \quad p(A) = \text{Intelligibility level} = \frac{937}{1,200} = 0.781$$

$$p(B) = 1 - p(A_r) = \frac{263}{1,200} = 0.219$$

$$2. \quad p(R_1/A) = \frac{705}{937} = 0.752$$

$$p(R_2/A) = \frac{112}{937} = 0.120$$

etc.

$$3. \quad P(R_c/A) = \sum_j^c p(R_j/A)$$

$$4. \quad p(R_1/B) = \frac{3}{263} = 0.011$$

$$p(R_2/B) = \frac{5}{263} = 0.019$$

etc.

$$5. \quad P(R_c/B) = \sum_j^c p(R_j/B)$$

$$6. \quad p(A/R_1) = \frac{705}{708} = 0.996$$

$$p(A/R_2) = \frac{112}{117} = 0.957$$

etc.

$$7. \quad p(R_1) = \frac{708}{1,200} = 0.590$$

$$p(R_2) = \frac{117}{1,200} = 0.097$$

etc.

Intelligibility

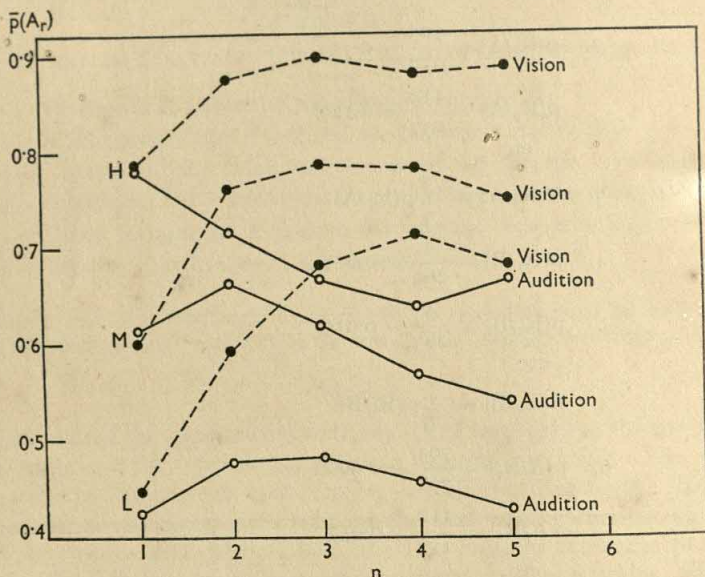
For the first trial and all repetitions within a given condition, the speech-to-noise ratio or the exposure duration remained the same. For audition the initial $\bar{p}(A)$'s were $H = 0.781$, $M = 0.613$, $L = 0.426$, where H , M , and L are used as convenient labels to mean High, Medium, and Low intelligibility levels. Similarly for vision, $H = 0.782$, $M = 0.600$, and $L = 0.446$. The mean intelligibility based on all six trials was for audition $L = 0.455$, $M = 0.612$, and $H = 0.731$; for vision the mean values were $L = 0.598$, $M = 0.695$, and $H = 0.825$.

The clearest differences to be found between the two modes of reception are in the changes in intelligibility $[\bar{p}(A)]$ over trials. (See Fig. 1.) For vision, the greatest increase is between trials 1 and 2 for L , M , and H . For M and H , $\bar{p}(A)$ is relatively constant over trials 2-5, while for L levelling off does not occur until the third trial. For condition L and M of audition, there is little change over trials, and this is consistent with previous results. But for condition H of audition, there is a

steady decline over trials 1-5, although the total drop is only about 12 per cent. The reason for this departure from the visual behaviour is not clear. It cannot be ascribed simply to heterogeneity since the $\bar{p}(A)$ values of these 60 words have very nearly the same variance as the 50 homogeneous spondee words used in several earlier studies.

It seems likely that the rise in visual intelligibility between trials 1 and 2, which is about double that generally found for audition can be attributed to the variability of the word relative to the fixation point on trial 1. The variability came from the

FIGURE 1



Shows the way in which $\bar{p}(A_r)$ changes over the first five trials. The dashed lines refer to visual reception and the solid lines to auditory reception. The parameter is the proportion of correct responding as estimated from the first presentations only, called intelligibility level. *H*, *M*, and *L* refer to high, medium, and low intelligibility level. See text.

fact that the apparatus did not allow for exact positioning of a word relative to the fixation point. Now, since the word's position was fixed over later trials, this variability disappears. In the case of low intelligibility (*L*) the word appears almost as a blur, and the difficulty of localization still exists on later trials.

The relation of accuracy of reception to rating categories

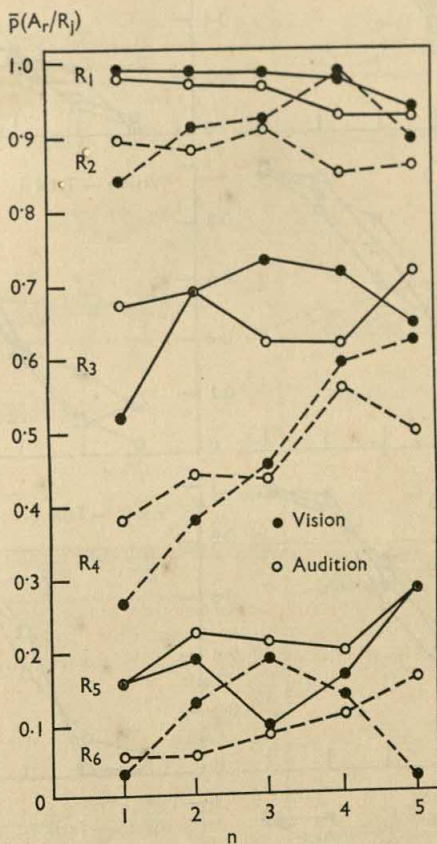
From Figure 2 it appears, for both modes of reception, that the probability of being correct within a high accuracy rating (R_1 or R_2), or within a low accuracy rating (R_5 or R_6), does not change remarkably over repetitions. However, accuracy within the two middle categories (R_3 and R_4) show much greater fluctuations over trials, and in fact, accuracy of reception within R_4 rises almost linearly from trial 1 to trial 5 at about 5 per cent. per trial. Again, the effects are greater for vision. (The functions for auditions are very similar to those obtained by Pollack and Decker (1958) for three trials, and our results may be said to extend their findings to five trials, for the words of these experiments.)

It seems to us that the consistent increase of accuracy within R_4 is due to words having many confusers. The receiver is reasonably sure that the word is one of a

small set, but is not willing to give his response a high accuracy rating. He has thus reduced the size of the set that he is working within, with a corresponding increase, relative to the total set, of being correct.

Figure 3 shows accuracy of reception within accuracy categories, as a function of category, with intelligibility level as the parameter. A comparison over trials within a modality or between modalities for any given trial shows very little difference. We conclude that vision and audition are quite similar in that for both, the accuracy

FIGURE 2

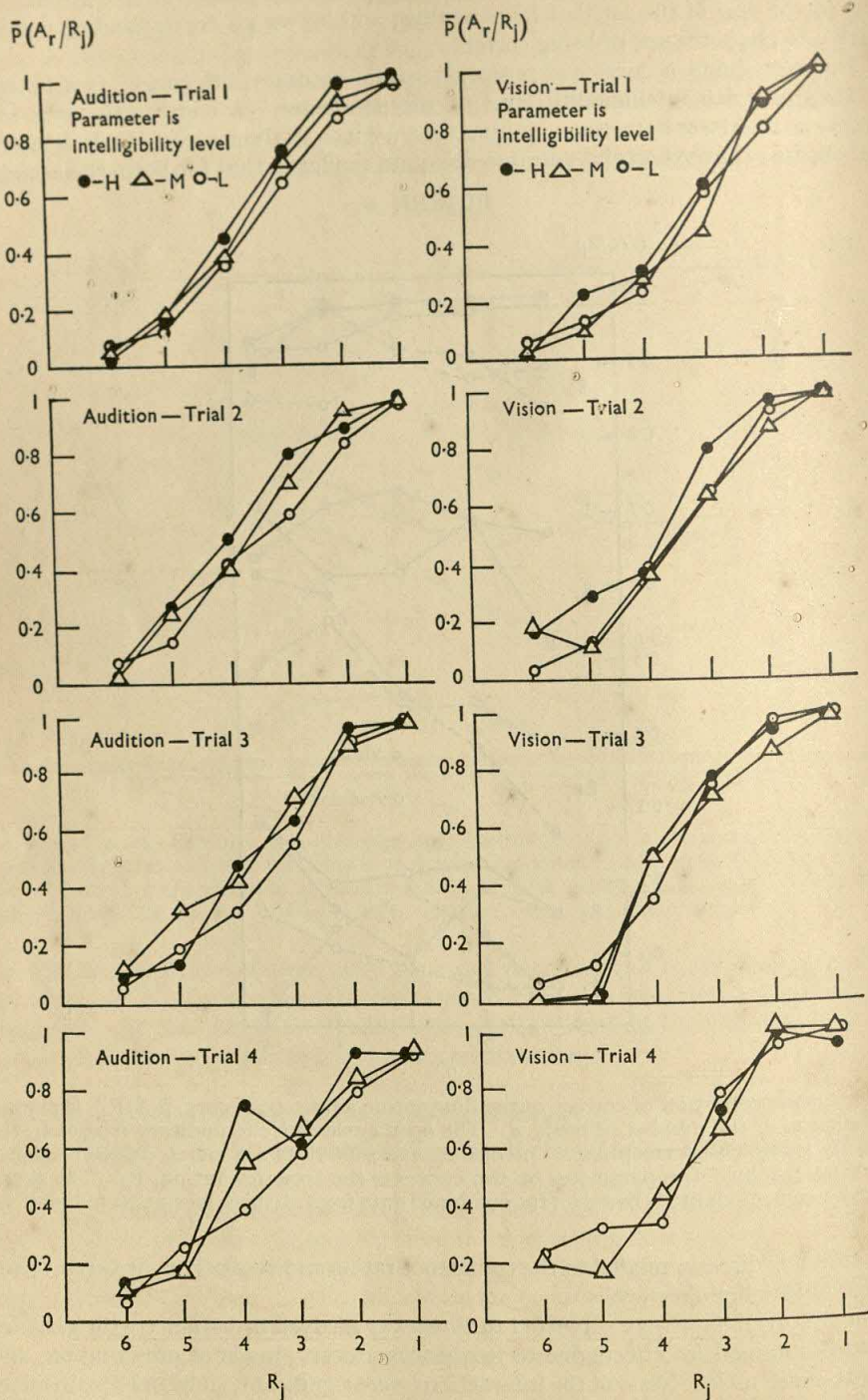


The mean proportion of correct responding within a rating category, $\bar{p}(A_r/R_i)$, is plotted as a function of the number of trials, n . The open circles refer to auditory reception, the closed circles to visual reception. The dashed and solid lines are merely for convenience in reading graphs. The parameter on the curves is the accuracy rating, R_i . The set of curves for each modality is based on the combined data from all three levels of intelligibility.

of reception is directly related to the confidence rating and is relatively invariant over a range of intelligibility levels.

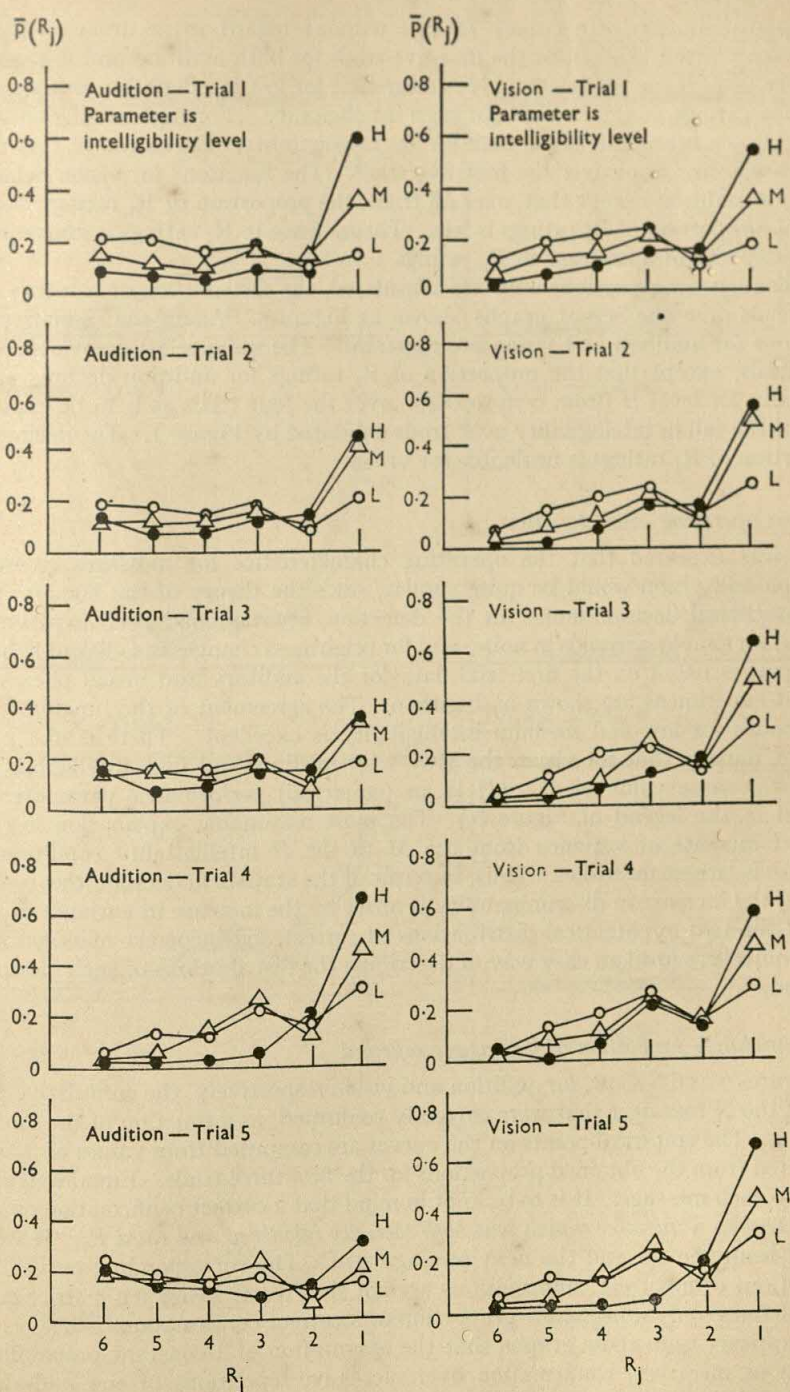
The first trial data were subjected to three-way analysis of variance with variance estimates obtained for effects due to frequency-category, mode of presentation, and signal-to-noise ratio. None of the interactions were significant. The only main effect was due to signal-to-noise ratio and was significant at beyond the 0.001 probability level, but, of course, this effect was experimentally built in.

FIGURE 3



The data of Figure 2 are replotted as a function of R_j with intelligibility level as the parameter. For ease of comparison each of the first four trials is plotted separately for both audition and vision. For H only of vision, the number of messages put in categories R_6 on trial 3, and in R_4 , R_5 , and R_6 on trial 4, were too small to compute stable proportions ($n < 100$).

FIGURE 4



The visual and auditory modes of reception are compared with respect to the distribution of accuracy ratings over trials. While the functions retain their form over trials, note that for vision the total numbers of messages transmitted on the fifth trial were less than a hundred for M ($N = 94$) and L ($N = 35$); the number of messages sent at the corresponding intelligibility levels for audition were 227 and 120.

The distribution of category ratings over trials

The distribution of accuracy ratings without regard to accuracy of reception have been plotted (Fig. 4) for the first five trials for both audition and vision. Intelligibility level is the parameter. It is clear that for low intelligibility levels more *low* accuracy ratings are given than for high intelligibility. For high intelligibility more *high* accuracy ratings are given than for low intelligibility. Furthermore, this relation is relatively invariant over the first five trials. The functions for vision behave like those for audition except that, over all trials, the proportion of R_1 ratings is greater, and the proportion of R_6 ratings is less. The increase in R_1 ratings is almost entirely at the expense of a decrease in R_6 ratings.

When correct receptions only are considered, the distribution of category ratings over trials give the set of graphs shown as Figure 5. Again the similarity of the functions for audition and vision are apparent. The same relative invariance holds over trials, except that the proportion of R_1 ratings for audition declines steadily, especially for level *H* (from 0.76 to 0.46), over the four trials as is to be expected in view of the fall in intelligibility over trials exhibited by Figure 1. The decline in the proportion of R_1 ratings is negligible for vision.

Receiver operating characteristics

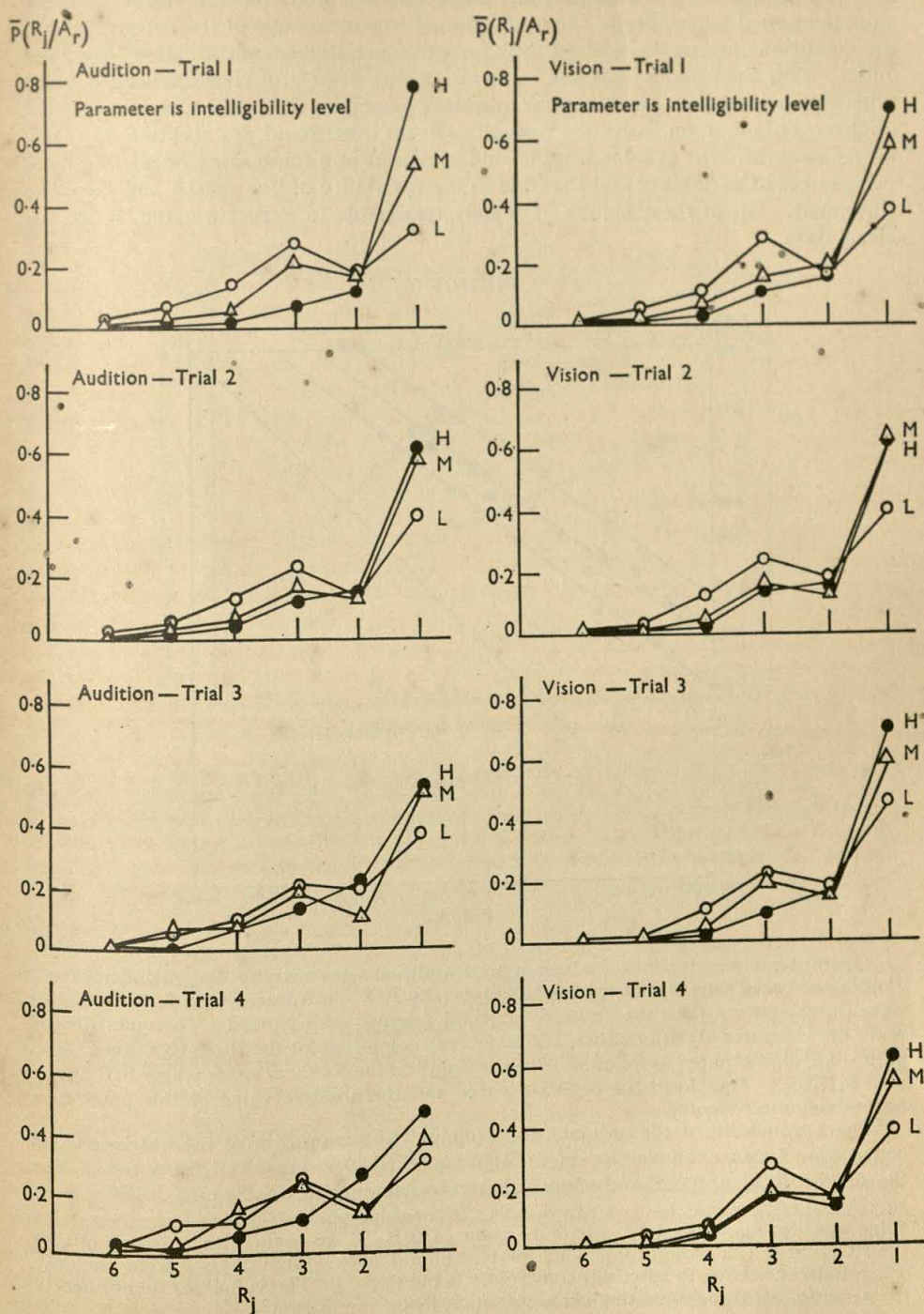
It was expected that the operating characteristics for messages received by audition and vision would be quite similar, since the theory of the human operator as a statistical decision-maker in the detection situation had been experimentally verified for simple sinusoids in noise, and for brightness changes in a disk of light. The ROC curves based on the first trial data for the auditory and visual phases of the present experiment are shown as Figure 6. The agreement of the functions for the two modes for low and medium intelligibility is excellent. There is one puzzling feature, namely that for vision the ROC's for medium and high intelligibility have about the same value of d'_e . (d'_e is an (empirical) performance parameter and is defined in the legend of Figure 6.) The most reasonable explanation lies in the marked increase of variance from the *M* to the *H* intelligibility condition. The increase is largest for vision. Thus, in terms of the statistical decision theory of ROC curves, the increase in discriminability is offset by the increase in variance in at least one of the two hypothetical distributions of correct and incorrect messages. This is only conjecture until an easy way of describing the distributions of such message sets is found.

The cumulative proportion of messages confirmed

Figures 7 and 8 show, for audition and vision respectively, the cumulative proportion of the *N* messages that were correctly confirmed, as a function of the number of trials, *n*. The empirical points on the curves are computed from values of p_y and p_{ec} estimated from the obtained proportions for the first three trials. Equal weights were given to each message. It is to be kept in mind that a correct confirmation on a given trial refers to a message which was *both correctly identified and rated R_1* . It was then retired from the set and the next message sent. The corresponding curves for the proportions of incorrect confirmations are not shown for, with such a strict criterion as R_1 defines, only a negligible proportion of incorrect confirmations occur.

It appears from these graphs that the assumption of a constant probability of a (correct or incorrect) confirmation over successive repetitions of one and the same message is not an unreasonable one. Perhaps the most surprising feature is that the visual data on the whole fit the theory rather better than the auditory data. With a

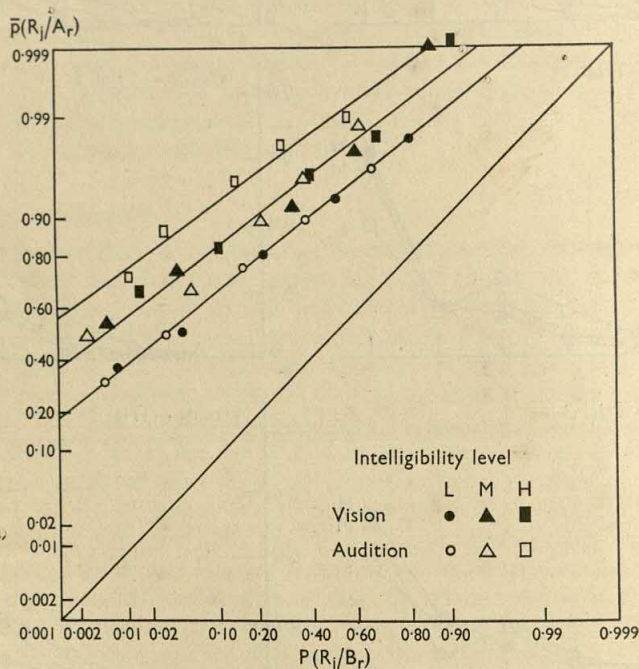
FIGURE 5



Vision and audition are compared over trials 1-4, for the mean proportions of accuracy ratings placed in the j th category, $\bar{p}(R_j/A)$, as a function of category rating, R_j . The parameter is intelligibility level.

little thought, it is not so surprising. Except for the first trial for vision (with the aforementioned variability of the location in the tachistoscope of the exposed word), the conditions are more homogeneous for the successive presentations of the word under vision than under audition. For vision, the word is of constant length, being typewritten it always has the same physical properties, and the variability in the brightness of an argon flash tube is small. On the other hand, for audition, the same words have different syllabic lengths, and were sent in a fluctuating noise. To these two sources of variability add that due to the variability of live speech and earphone placement. Given these factors, it is only reasonable to expect a closer fit for the visual data.

FIGURE 6



Operating characteristics for vision and audition obtained by the rating method. Points are based on the first trial data only. The ROC lines were fitted to the *auditory* data (open figures), then the visual data (solid figures) were plotted. The quantities d'_0 and s are measures of performance, and have been computed for the three ROC lines. The value of d'_0 is computed as follows: Take the point on the ROC curve for which $P(R_j/A) = 1 - P(R_j/B)$. Transform the ordinate value and the abscissa value of this point to a normal deviate (z -score).

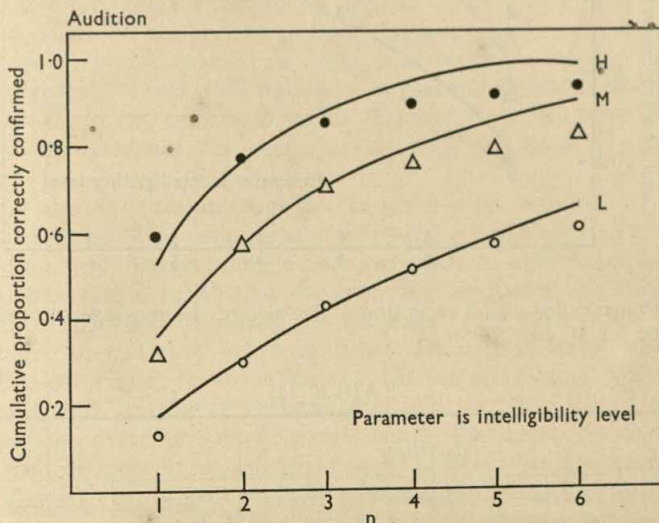
The z -transform of the ordinate value minus the z -transform of the abscissa value equals d'_0 . As an example: for $P(R_j/B) = 0.20$, $P(R_j/A) = 1 - P(R_j/B) = 0.80$. For the middle ROC curve the abscissa value corresponding to 0.80 is 0.07. Then, $d'_0 = z(0.80) - z(0.07) = 0.85 - (-1.43) = 2.88$. If the slope of the ROC curve were one, this value would remain constant, whatever value of $P(R_j/B)$ we begin with. Assuming that the ROC curve is a straight line when plotted on a double normal deviate scale, the only other number needed to specify it completely is the slope, s . The value of s is very nearly the same for all three operating characteristics, 0.86.

For a discussion of the performance parameters d'_0 and s , see the paper of Clarke, Birdsall and Tanner (1959).

In any case the data are further support of the independence assumption as being a good engineering approximation in the computation of various average quantities associated with the transmission and confirmation of messages under the operational rules we have specified.

The computations of $T_{ec}(5)$ and $T(5)$ were made by Equations (9) and (10). $T_{ic}(5)$ is just $p_{ic} T(5)$. All of these statements are based on the independence assumptions, as discussed in the Introduction.

FIGURE 7



The cumulative proportion of messages correctly confirmed, $p_{ec}(n)$ is shown as a function of the number of trials, n , for auditory reception. The cumulative proportion of messages *incorrectly* confirmed is not plotted, for with R_1 defining a rather strict criterion of acceptance, $p_{ic}(n)$ is very small (see Table III under $T_{ic}(n)$). The parameter is intelligibility level. The size of the message set was 1080 for conditions L and M , and 1200 for H . Each test item was sent to the receiver until he confirmed his identification of the word (rated it R_1), or until six trials had occurred. When a rating of R_2 or greater was assigned the receiver knew that the same item would be sent again.

Estimates of the probability of a confirmation, p_y , and of the probability of a correct confirmation, p_{ec} , were made from the data for the first three trials only. All messages were given equal weights. The curves shown in the figures are plotted from.

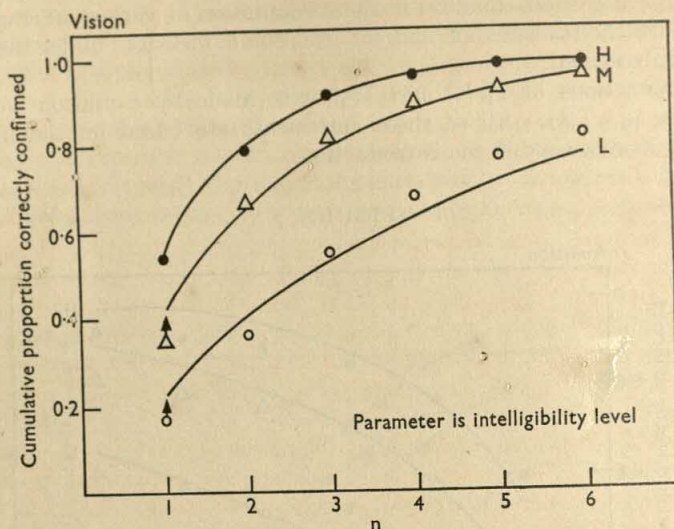
$$p_{ec}(n) = p_{ec}(1 - p_y)^n \quad \dots \quad \text{Equation (12)}$$

which is just Equation (9) divided by N .

The expected number of communication events

Under repetition with a criterion of acceptance or rejection, the little theory outlined in the Introduction makes some other predictions. Among these are the total number of communication events that will have occurred up to and including a given trial, $T(n)$, as well as the total number of correct and incorrect confirmations, $T_{ec}(n)$, and $I_{ec}(n)$, respectively. These data are shown as Table III for $n = 5$. In comparing theory with data, the probability of the largest Chi-square obtained was greater than 0.80 (for $T_{ec}(5)$). There is no doubt that the predicted numbers are in excellent agreement with those obtained experimentally.

FIGURE 8



Same as Figure 7 for visual reception. The size of the message sets was 1080 for *L* and *H* and 960 for *M*.

TABLE III

Intelligibility (N = 1,200)	Audition			Vision		
	Low	Medium	High	Low	Medium	High
T(5)	3,827 (3,880)	2,720 (2,764)	2,263 (2,260)	3,423 (3,463)	2,150 (2,166)	1,906 (1,909)
T _{cc} (5)	639 (612)	925 (863)	1,179 (1,102)	773 (829)	888 (896)	1,055 (1,059)
T _{ic} (5)	18 (13)	29 (24)	10 (9)	13 (10)	8 (7)	7 (6)

Showing (1) the total number of message units sent up to and including the fifth trial, *T*(5); (2) the cumulative number of the *N* messages correctly confirmed up to and including the fifth trial, *T_{cc}*(5); and (3) the cumulative number of messages incorrectly confirmed up to and including the fifth trial, *T_{ic}*(5). The quantities in parentheses were obtained experimentally.

DISCUSSION

Let us say again, briefly, what this study has shown. The rating method was used to obtain operating characteristics for 60 heterogeneous words, and to make specific comparisons of the visual and auditory modes of reception. A single message was sent under difficult conditions of reception and was repeated until it had been assigned to the highest accuracy category ("confirmed") or until it had been sent a maximum of six trials. The comparisons made showed that it matters little whether reception is by eye or by ear. Whether within a trial or over successive repetitions, accuracy of reception is a direct function of the confidence rating and is relatively independent of the intelligibility level. Neither do the accuracy of reception nor the distributions of

rating categories change markedly over trials beyond the first. Although no direct test was made, it appears that accuracy of reception is not lessened by the task of rating.

Again, both visual and auditory data are fitted reasonably well by predictions made from a simple stochastic model based on the assumptions that (1) intelligibility, (2) probability of a correct acceptance and (3) probability of an incorrect acceptance remain constant over successive repetitions.

The main contribution of this paper is to show that the concept of operating characteristic as used in the reception of messages in noise may be extended to include vision. Furthermore, these experiments suggest to us that any theory which is adequate to account for the complex discriminations found in the language-bound behaviour of speech perception will go a long way toward accounting for visual perception of language. These two modes of perceiving involve disparate physical receptor systems, but the associated verbal and motor systems must be intimately connected. Some evidence for central processing has been mentioned in the Introduction.

A last word about the assumption that the receiver operates as if he were playing a Bernoulli trials game: The assumption of independence of trials is of great antiquity in psychophysics, yet there is strong evidence against it. For example, Verplanck, Collier and Cotton (1952) found that the successive responses of a subject given in measurements of his visual threshold are far from being statistically independent of one another. In fact they found significant serial correlations on time-ordered sequences of responses out to about lag 11. On the other hand, Miller, Heise and Lichten (1951) found that the articulation score rises very little after the first two of a series of successive presentations of the same word. Postman and Adis-Castro (1957) suggested that tachistoscopic recognition of words proceeded as if successive exposures were independent. In the present paper we examined the validity of the independence assumption and found that while not strictly true, it makes a good "engineering" assumption. In a recent paper, Swets and Green (1961) applied Wald's theory of sequential analysis in extending the statistical decision model as applied to the detection by human observers of signals in noise. They explicitly considered whether or not the observer integrates the successive observations on a sequence of intervals. The sequence was terminated only by a *Yes* (signal present) or *No* (signal not present) response. Their analysis "leaves little doubt that, . . . the assumption of no integration (i.e. independence) over successive observations is a good one." However, in another experiment in this series, explicit instructions were given to the observers to attempt to integrate over successive observations, and the evidence was that they could indeed do so.

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All of the data from this experiment are being analysed for sequential dependencies and message confusability on the IBM 7090 computer. We should like to acknowledge the help of IBM Western Data Processing Centre, University of California, Los Angeles, in programming and processing the data.

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COMMUNICATION THEORY AND THE CONSTELLATION HYPOTHESIS OF CALCULATION

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It is assumed that in translation tasks the brain behaves as an information-processing system for which the time-requirement (t) of a translation is strictly proportional to the information which must be handled in performing it. Writing $Q[\text{calc.}]$ for the information-requirement of a given calculation, the problem is to define a procedure Q which may be applied to calculations involving various arithmetical operations on numbers of varying magnitudes, such that $Q[\text{calc.}] = k.t$. The constant k would then give the information-handling rate.

Experiments with six subjects on seven types of simple addition-sum lead to a definition of Q which satisfies this condition for 25 out of 42 observations. Of the other 17 observations, 14 are accounted for by a modified definition of Q and at least one more on the assumption that "redundant information" may be handled in some cases. The main hypothesis, with these two subsidiary assumptions, accounts well for all observations in a further experiment with four subjects on six types of addition, subtraction and multiplication.

The definition of Q is next generalized by analogy to deal with more complex multiplications. A prediction concerning multiplications of the type $[x \cdot \log x]$ is borne out by 11 out of 12 subjects and the subsidiary assumptions are not required. Information-rates found by this rapid method range from 2.07–5.78 binary units of $Q[\text{calc.}]$ per second (BU/sec.) and there is reason to believe that 1 BU is equivalent to 1 bit.

Communication theory is gradually finding a place in the study of mental processes and in view of the systematic nature of the concept of Number, quantitative methods derived from information theory might be expected to be applicable, if nowhere else, to the analysis of the mental and neurophysiological processes involved in arithmetical calculation. There seems to be little in the way of experiment or theory in this field and no intensive study appears to have been made of the relation between the arithmetical magnitude of a calculation and the time required for its performance. An essential part of any such investigation must clearly be the definition of some exact and if possible neurophysiologically meaningful criterion of "arithmetical magnitude"; it is here that information theory might be expected to contribute.

Formulation of the problem

The preliminary experiments to be discussed below were originally carried out as an exercise in the use of "controlled series" with the seemingly trivial aim of finding out, by comparing the average times required for different types of addition, how much time, if any, is taken up by the act of "carrying one." Implicit in this way of putting the problem was the assumption that a calculation such as $37 + 29 = 66$ is carried out in a consecutive series of discrete steps, of which the "carrying" is one. This "step-by-step" hypothesis had to be abandoned since it yielded grossly inconsistent and implausible values for the "carrying-time" and other hypothetical step-times. It was therefore decided to seek some way of expressing the results in terms of information theory.

Information is usually defined statistically with reference to the relative frequencies with which different types of signal occur in the message, are uttered by the source, or are presented to the recipient considered. However, another approach seems to be

more readily applicable to the problem of calculation. In general, the time taken by a communications system to transmit a given message is strictly proportional to the information-content of the message, if the latter is suitably coded. Under these conditions, the information-contents of different messages could in principle be compared by comparing the times taken to transmit them through a suitable communications system. Furthermore, a straightforward extension of the same principle renders it applicable to devices which do not merely transmit but also translate messages; namely, that *the time taken to perform a given translation will be strictly proportional to the amount of information which the system must handle in order to perform it*. Provided that the information-handling rate of the system remains constant, the time-requirements of different messages will be strictly proportional to their information-requirements and the constant of proportionality will measure the information-handling rate of the system. Unless assumptions similar to those of this paragraph are made, it is difficult to see how time-measurements could be used to infer the information-requirements of different calculations.

Now the simplest calculations are those such as $2 + 3 = 5$, or $7 \times 8 = 56$, which we may term "one-stage" and in which two one-digit numbers are subjected to a simple arithmetical operation. Such sums are conveniently expressed by the general equation $O(p,q) = r$, where O denotes the operation performed on the problem-digits (p,q) and r is the result. This representation recalls that calculation is a strict form of translation: the left-hand side $O(p,q)$ is translated into the right-hand side r , both sides having exactly the same meaning owing to the tautologous definitions of arithmetical symbols. In performing the calculation, the brain receives a specific input message which conveys the concept $O(p,q)$ and in due course produces an appropriate output which conveys the equivalent numerical concept r . We may usefully think of $O(p,q)$ as an input-message, or "conceptual stimulus," the correct translation of which calls for the handling of a certain amount of information by the brain.

The same approach holds good for more complex arithmetical tasks in which the result is not derived from the input-message in one, but in several stages. However, in such cases there may be the possibility of choice; the result can often be derived by more than one route and it may be that the information-requirement of a given calculation will depend upon the route adopted. This possibility is quite compatible with the axiom as stated in its modified form.

Ignoring such complications for the moment and assuming that under the present experimental conditions at least, the brain behaves as a communications device with a constant information-handling rate, our problem becomes that of seeking some exactly definable procedure which, when applied to a given calculation, will yield a number strictly proportional to the observed time taken to perform the calculation. Ideally, the procedure should be universally applicable, but from what has just been said it will be clear that the same procedure for computing the information-requirement may not always be valid. In formal terms, denoting the desired procedure by the operator symbol Q , our object is so to define Q that for a series of calculations by a given subject $Q[\text{calc.}] = k.t$, where t is the time taken to perform a calculation and $Q[\text{calc.}]$ is the number obtained by applying the operator Q to the calculation. If this aim were achieved, $Q[\text{calc.}]$ could be regarded as the amount of information handled by the subject in accomplishing the calculation in question; the constant k could be estimated graphically by computing $Q[\text{calc.}]$ and measuring t for a variety of sums; and the value of k would indicate the information handling rate of the subject. It would, however, remain to be shown that information measured by this indirect method was expressed in the same units as the information which is statistically defined.

Preliminary experiments

Supposing that some general definition of the operator Q has been postulated, then in order to test for the existence of the predicted strict proportionality between the observed time-requirement (t) and the theoretically computed information-requirement $Q[\text{calc.}]$, it is clear that each experimental subject should be made to perform calculations covering as large a range of these variables as possible. Again, the greater the variety of arithmetical situations encompassed by these calculations the more comprehensive, in another sense, will be the test of the proposed definition. The method adopted in the original experiments was to present the subject with thirteen rows of calculations comprising seven different types, the sums in any row being numerically as diverse as possible within set limits, but arithmetically comparable in other respects. According to the definition of Q discussed below, these calculations span a threefold range of $Q[\text{calc.}]$.

The use of this method means that average times and average information requirements, for an "average calculation" representative of each row, will constitute the experimental data, rather than exact values for individual calculations. This theoretical weakness is likely to be compensated by the smoothing-out of minor fluctuations in performance. The technique also avoids what was felt at the outset to be a serious dilemma posed by the alternative method of timing single calculations, namely that if timing is to be accurate the numbers must be large and the different possible ways of analysing each calculation then become discouragingly numerous.

We shall now consider the rows of number-pairs employed and introduce the system of notation used to denote the various types of calculations which subjects were made to perform on the different sorts of row.

Take first the addition of two numbers such as 29 and 48—an addition with carrying, having a two-digit answer. We shall refer to such sums generally by writing the added numbers (ax) and (by), where the letters stand for single digits such that $(x + y)$ always exceeds 10, and $(a + b + 1)$ never exceeds 9. Since the vertical pair of digits (a , b) lies to the left of the pair (x , y), number-pairs of this kind will be coded as $abxy$. Since the same row of number-pairs may be used for more than one type of calculation on different occasions the coding of a row of calculations will also include a symbol to indicate what arithmetical operation has been performed, e.g. $abxy +$, $abxy -$. The experimental Sheet 1 bore two rows of fifteen $abxy$ sums.

In the rows coded $xyab$ the same rules govern the values of the digits but the order of the vertical digit-pairs in each pair of two-digit numbers is reversed. The numbers are of the form (xa), (yb) and an $xyab +$ is therefore an addition without carrying, having a three-digit answer, such as: $92 + 84 = 176$. Two rows of fifteen $xyab$ number-pairs were made up.

Rows of paired single-digit numbers were also prepared, the digit-pairs being either of the ab or the xy type. It will be clear that such rows can be used for addition, subtraction or multiplication and that they enable the same one-stage operation to be applied to digit-pairs of two different average magnitudes. Forty-five ab and forty-five xy digit-pairs were made up and presented as two rows of each sort, containing 22 and 23 sums respectively.

Two special arithmetical programmes, coded $xy10$ and $ab1$, were devised for application to xy and ab rows respectively, with the step-by-step hypothesis in mind; they proved, however, to be profoundly relevant to the constellation hypothesis also. The first two discrete steps in doing an $abxy +$ sum might be imagined, on the step-by-step hypothesis, to be (i) Add x and y , (ii) Take 10 from $(x + y)$. It was originally supposed that the second of these constitutes one part of the process known as "carrying one" and it was hoped that the time required for this step might be deducible by comparing the time taken to perform a simple $xy +$ addition with the time taken to perform, on the same pairs of digits arranged in different combinations, the $xy10$ operation of first adding x and y and then subtracting 10 before giving the answer. In the same way, the time-requirement of the second part of "carrying one" was to be measured indirectly by comparing the simple $ab +$ addition with the $ab1$ programme, in which the subject first adds a and b and then adds 1 to their sum before giving the answer.

Finally, another type of row comprising pairs of two-digit numbers was made up. Both numbers were of the form ab and a row of this type may be coded $abab$. An $abab +$ sum is therefore an addition without carrying, having a two-digit answer, e.g. $24 + 63 = 87$. There was room on the page for only one row of twenty $abab$ sums.

In making up the rows certain precautions were observed. The various rows were interleaved. To avoid possible ambiguities care was taken never to place two equal digits one above the others; zero was not used; and numbers were randomized as far as possible

within the special restrictions. Table I gives a schema of the mimeographed typewritten sheet (Sheet 1) presented to each subject, together with several columns of quantitative data concerning the performance of various arithmetical operations on each of the rows.

TABLE I

SCHEMA OF SHEET 1 WITH VALUES OF MEAN CONSTELLATION-INFORMATION IN DECIMAL UNITS OF $Q[\text{calc.}]$

Type of row	No. of sums in row	Mean $Q[\text{calc.}]$ for various operations on the row					
		+ L	- L	$\times L$	$ab1L, xy10L$	$ab1S, xy10S$	+ S, - S, $\times S$
ab	22	1.11	0.95	1.20	1.33	1.18	0.81
abxy	15	2.95	—	—	—	—	2.62
ab	22	1.10	0.95	1.20	1.33	1.18	0.80
xyab	15	2.53	—	—	—	—	1.93
xy	22	1.45	1.24	1.78	1.59	1.42	1.15
xy	23	1.47	1.24	1.85	1.65	1.47	1.17
ab	23	1.12	0.94	1.22	1.34	1.18	0.82
abxy	15	2.98	—	—	—	—	2.65
ab	23	1.12	0.94	1.22	1.34	1.18	0.82
xyab	15	2.59	—	—	—	—	1.99
xy	23	1.47	1.24	1.83	1.65	1.47	1.17
xy	22	1.45	1.24	1.78	1.59	1.42	1.15
abab	20	2.24	—	—	—	—	1.64

Suffix L denotes the assumption of a "long" and suffix S , that of a "short" constellation.

Each subject was told to perform each row as fast as possible, consistent with making no mistakes. However, he was also instructed *not* to falter or repeat a calculation, should he think he had committed a mistake. Many subjects show a tendency to pause for a moment about the middle of a row, but the row is not regarded as invalid unless the subject obviously breaks down and disengages his attention from the task. The arithmetical operation required is announced at the start of each row; with pencil at the ready under the first sum, the subject is then given the signal "Are you ready? Go!" and timed with a stopwatch. The timing error is estimated to be always less than 0.02 sec. per sum.

One subject (B. D.), in a special experiment, performed two or more rows each of $ab +$, $ab -$, $ab \times$, $xy +$, $xy -$ and $xy \times$ among others. Six other subjects each worked through Sheet 1 and did two rows of $ab +$, $ab1$, $xy +$, $xy10$, $abxy +$ and $xyab +$ and one row of $abab +$. Two of these subjects (R. J. M. G. & H. B. G. T.) gave spoken answers but the rest wrote their answers on the experimental sheet.

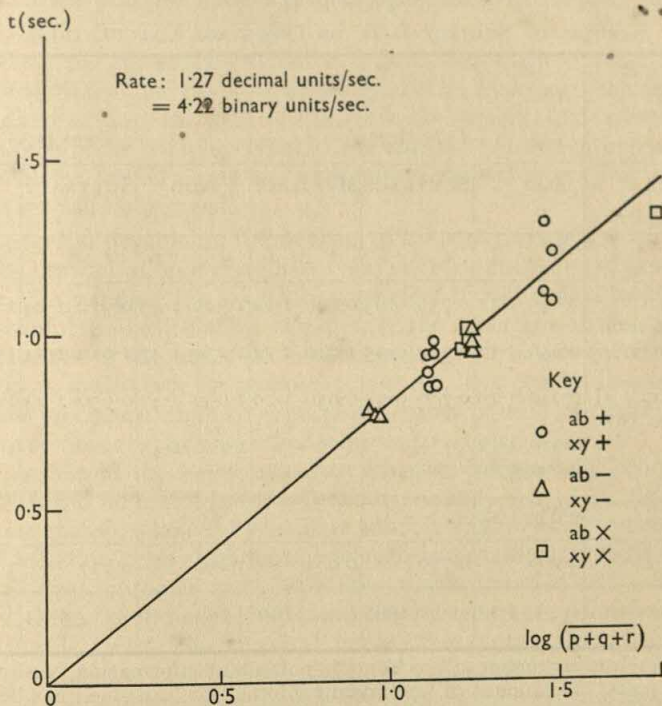
The Constellation hypothesis

After several unsatisfactory attempts to define the nature of the relation, if any, between the characteristics of a calculation and the time required to perform it, a clue was given by a special experiment in which one subject (B. D.) did a long series of

one-stage calculations, comprising addition, subtraction and multiplication on ab and xy rows. It was found empirically that for 20 out of the 23 rows performed, a remarkably simple relationship existed.

As already remarked, such calculations may all be represented by the general equation $O(p,q) = r$. When the average time per sum for a row was plotted against the quantity $\log(p + q + r)$, the number $(p + q + r)$ having been averaged for the row, a strict proportionality of the form $\bar{t} = 0.79 \log(p + q + r)$ was found. In Figure 1 each point represents one row of sums; if the mean results for like rows are

FIGURE I



Performance of subject B. D. on ab and xy digit pairs, with three different arithmetical operations (see Table II).

plotted the fit is extremely close. All the rows have been plotted, in Figure 1, for $ab +$, $ab -$, $ab \times$, $xy +$ and $xy -$; but for reasons to be discussed later only the last (fourth) row of $xy \times$ is plotted. However, the entire data are given in Table II.

It will be seen that the sole feature of the calculation which affects the time per sum in this instance is the collective magnitude of the numbers involved. The nature of the operator O in $O(p,q) = r$ only affects the time in the sense that it specifies, according to the tautologous conventions of arithmetic, what number r must be associated with the given numbers (p,q) on a given occasion. This point is especially well illustrated by the results of $ab \times$ and $xy -$, for which the times and the values of $\log(p + q + r)$ are very similar although the operations and the problem-digits are different.

Taking the average law for the row to reflect the average handling of individual calculations in that row, it seems that in this experiment the time taken to "translate"

$O(p,q)$ into r is strictly proportional to $\log(p + q + r)$. According to the introductory formulation of the problem, this suggests that the information-requirement of the calculation $O(p,q) = r$, which we may write $Q[O(p,q) = r]$, is given by $\log(p + q + r)$. If so, then the subject's rate of handling information must be the reciprocal of 0.79, i.e. 1.27 units per second.

The fundamental question now arises as to the nature and magnitude of these units. If the general assumptions put forward in the introductory formulation are correct, the units must be measuring information, in the broad sense that "information" is the attribute of a message which causes its transmission to require time.

TABLE II
RESULTS OF SUBJECT B. D. ON ONE-STAGE CALCULATIONS

Input digit-pairs (p,q)	Type (a,b)						Type (x,y)					
	ADD +		SUBTRACT -		MULTIPLY ×		ADD +		SUBTRACT -		MULTIPLY ×	
Operation O												
	t	I	t	I	t	I	t	I	t	I	t	I
Serial values of: t = time per sum, mean of a row.	0.845	1.11	0.759	0.95	0.990	1.22	1.320	1.45	0.970	1.24	2.290	1.78***
	0.940	1.12	0.770	0.95	1.010	1.22	1.230	1.47	1.010	1.24	1.790	1.78**
$I = \log(p + q + r)$ mean of a row where: $O(p,q) = r$	0.940	1.10	0.780	0.94	0.950	1.20	1.090	1.47	0.950	1.24	1.790	1.85*
	0.970	1.12	—	—	—	—	1.120	1.45	—	—	1.340	1.78
	0.890	1.10	—	—	—	—	—	—	—	—	—	—
	0.850	1.12	—	—	—	—	—	—	—	—	—	—
Mean t	0.906		0.770		0.983		1.192		0.977		1.803	
Mean I	1.11		0.95		1.21		1.46		1.24		1.80 (2.25)	

Note: Asterisks denote number of bits/sum of "redundant information" assumed. In the case of (2.25) the amount of "redundant information" assumed has been averaged over all four rows performed and added to 1.80.

Moreover, since logarithms to base 10 have been used here in computing $Q[\text{calc.}]$, rather than logarithms to base e or to base 2, the units must in some sense be decimal, rather than "natural" or binary, and may be converted to binary units (for example) by dividing by 0.3010. The simplest position to adopt would be that an information-handling rate deduced in the manner described is measured in absolute decimal units of information per second. However, the technique employed does not exclude the possibility that there may be a constant u , such that one decimal unit of $Q[\text{calc.}]$ is equivalent to u decimal units of information defined in absolute terms. There is in fact some reason to believe that $u = 1$, as will appear below, so that $Q[\text{calc.}]$ does seem to give the information-requirement in absolute units of information, but in view of the residual doubt, rates deduced by the calculation method will be expressed in DU/sec. In the same way rates or quantities derived from the application of the operator Q and computed with logarithms to base e or to base 2 will here be expressed in "natural units" or "bits," written NU and BU respectively.

The linear relation of Figure 1 is obeyed with a precision which is perhaps surprising,

since no allowance has been made for "learning" or "practice" effects. However, in the case of $xy \times$ the time per sum decreases markedly in successive rows (Table II) and as the values of $\log(p + q + r)$ are greatest of all for the $xy \times$ rows this may be a progressive practice effect. In fact, if to the values of $\log(p + q + r)$ computed for the 1st, 2nd, 3rd and 4th rows performed, are added respectively 3 bits/sum, 2 bits/sum, 1 bit/sum and 0 bit/sum (in absolute units, i.e. the quantities 0.90, 0.60, 0.30 and 0 respectively) then the four points for these four rows all come to lie closely about the line of Figure 1. This suggests that in certain circumstances, superfluous information is handled (or the equivalent amount of time allotted) initially, but may be discarded with practice until the time-requirement is uniquely determined by $\log(p + q + r)$.

Now in order to explore the generality of the finding $\log(p + q + r) = k.t$, it will be necessary to find some way of expressing it as an hypothesis which can be subsequently tested against the data of the other six subjects and eventually applied systematically to more complex types of calculation. Any such hypothesis will, of course, have both a formal aspect and a neurophysiological aspect, but for the present only the former will be pursued.

From the formal standpoint, the meaning of the equation $\log(p + q + r) = 1.27 t$ appears to be that for at least one subject and occasion the required procedure Q has been found. To compute the information required to be handled by this subject in order to accomplish a calculation $O(p, q) = r$, the procedure evidently consists in *taking the logarithm of the sum of the numbers which occur in an explicit representation of the calculation*, in this case the numbers p , q and r . This conclusion at once suggests that the same procedure might be applicable to other types of calculation, to account formally for the time required by other subjects to accomplish them.

The proposition of the foregoing paragraph may conveniently be expressed in a notation which will be useful later, as $Q[O(p, q)] = \log(p + q + r)$. We shall adopt the convention that Q , followed by a square bracket which specifies either a particular numerical calculation or an algebraically defined class of calculations, is to mean "the standard information-requirement of" the calculation denoted. For example $Q[7 - 2]$ will mean "the standard information-requirement of performing $7 - 2 = 5$ " and $Q[X.X^2]$ will mean "the standard information-requirement of multiplying a number X by its square." The word "standard" is inserted both as a reminder of our uncertainty as to the absolute magnitude of Q -units of information and also in order to indicate that the subject considered is assumed not to be adopting any special mode of performance, such as the handling of superfluous "redundant information" or extra time, or any unconventional procedure for calculating.

In the above equations the letters p , q and r stand, not merely for numerical symbols, but for quantitative numerical concepts, true cardinal numbers. We shall require a term for clusters of numbers such as (p, q, r) and at this point the notion of a "constellation" will be defined. *Where any set of numbers $(n_1, n_2, \dots, n_w)$ occurs in the course of a calculation and where the time taken to perform this calculation, relative to others, is proportional to $\log(n_1 + n_2 + \dots + n_w)$ this set of numbers will be called a constellation.* In this paper the letter Z will be used to refer to the sum of the numbers in a constellation. Any representation of the calculation concerned, which employs the numbers $(n_1, n_2, \dots, n_w)$ and only these numbers, will be called the *constellation-diagram* of the calculation as performed by the subject in question. The quantity $\log(n_1 + n_2 + \dots + n_w)$ will be called the constellation-information.

The next step consists therefore in searching by trial and error for constellation-diagrams to fit the performance of each subject on each type of calculation. In

practice it turns out that only two principles, one of which has to be modified for some calculations done by some subjects, are required in order to construct and combine constellation-diagrams which closely fit the performance of the seven subjects of these preliminary experiments, on nearly all the types of calculation done by them. The operator Q consists in applying these principles as appropriate so as to dissect the calculation into a series of constellations and then computing the total constellation-information, $\log Z_1 + \log Z_2 + \dots$, etc., according to the definition given above.

Constellation-diagrams for various types of addition-sum

Following the lead given by the results of subject B. D., the *first principle* applied to the results obtained with Sheet 1 was that the constellation-diagram for any simple arithmetical operation on a pair of one-digit numbers should contain all the numbers used in an explicit, or exhaustive, performance of the calculation. On this basis the "long" constellation-diagrams for all one-stage calculations of the type $O(p,q) = r$ would contain the numbers p , q and r and might be written $[p,q:r]$. In the same way the diagrams for $ab +$, $xy +$, $ab1$ and $xy10$ might be written $[a,b:(a+b)]$, $[x,y:(x+y)]$, $[a,b:(a+b), 1:(a+b+1)]$ & $[x,y:(x+y), 10:(x+y-10)]$ respectively, the square bracket in each case containing all the numbers which occur in proceeding from the digits of the problem to the final result. To take numerical examples, representing the calculations in a more familiar manner, we have:

$ab +$	2	7	$ab1$	2	7
4	9	4	$xy10$	4	9
$\hline$	$\hline$	$\hline$	$\hline$	$\hline$	$\hline$
6	16	6	16	1	10
		$\hline$	$\hline$	$\hline$	$\hline$
		7	6		

—printing the answer-digits in heavy type. Thus $Q[2+4] = \log(2+4+6)$ and so on.

Turning next to the calculations on pairs of two-digit numbers, the *second principle* assumed was that such calculations should be regarded as comprising two successive constellations, one for each vertical pair of digits. An $xyab +$ sum, which intuitively seems to consist of an $ab +$ stage followed by an $xy +$ stage, would therefore be represented: $[a,b:(a+b)].[x,y:(x+y)]$ and the operation Q would be completed by taking the logarithm of the sum of numbers in each square bracket and then adding the logarithms. This principle amounts to the assumption that the brain handles the two constellations separately and consecutively, and that the time taken is determined by the sum of the amounts of information associated with each constellation. To take a numerical example of an $abab +$ sum, $Q[34+52] = \log(4+2+6) + \log(3+5+8)$ or, equally, $\log(4+2+6) \cdot (3+5+8)$ viz. 2.283 DU.

The act of "carrying one" was included quite naturally under this principle by supposing that an $abxy +$ calculation should be regarded as comprising an $xy10$ stage followed by an $ab1$ stage. In general therefore $Q[abxy+] = Q[xy10] + Q[ab1]$ and for example, $Q[27+49] = \log(7+9+16+10+6) + \log(2+4+6+1+7)$.

Armed with these two principles the next step was to compute an average value of $Q[\text{calc.}]$ for each row of number-pairs on Sheet 1, by applying the appropriate arithmetical operation to each number-pair of the row and averaging the values of $(n_1 + n_2 + \dots + n_w)$ so as to obtain an average Z corresponding to a constellation representative of the entire row of calculations. The logarithm of this Z was taken as the mean $Q[\text{calc.}]$ for the row. For two-stage sums such as $abxy +$ this procedure

was carried out separately for the two stages and the sum of the logarithms of the two average Z 's was taken as $Q[\text{calc.}]$. Table I shows the results of applying the operator Q in this way, in those columns of which the heading has the subscript L . The letter L refers to the fact that standard "long" constellations have been assumed in the computation, according to the first principle already defined.

When the average time per sum was plotted against the average $Q[\text{calc.}]$, pooling the results for like rows, each of the six subjects yielded seven graph-points corresponding respectively to two rows each of $ab +$, $xy +$, abI , $xyIO$, $abxy +$ and $xyab +$, and one row of $abab +$. It was found that about half the points, for each subject, lay on or very close to a straight line through the origin as the theory required, but that most of the remainder lay off the line to one side, implying that the time per sum

TABLE III

APPLICATION OF THE CONSTELLATION HYPOTHESIS TO VARIOUS TYPES OF ADDITION

Type of calculation	Constellation assumed	Mean $Q[\text{calc.}]$	Mean times per sum (observed)					
			S. S.	J. A. F.	S. R.	J. H. L. B.	R. J. M. G.	H. B. G. T.
$ab +$	short	0.81	0.592	1.232	—	0.834	—	0.449
	long	1.11	—	—	0.563	—	0.451	—
$xy +$	short	1.14	—	—	—	—	—	0.659
	long	1.46	0.974	2.200	0.713	1.405	0.538	—
abI	short	1.15	—	1.644	—	—	0.440	—
	long	1.33	0.947	—	0.771	1.260	—	0.714
$xyIO$	short	1.45	—	2.116	0.753	1.307	—	0.794
	long	1.62	1.154	—	—	—	0.660	—
$abxy +$	short	2.60	1.807	—	—	—	—	—
	long	2.95	—	(4.847)	1.524	2.670	1.553*	(1.768)
$xyab +$	short	1.97	—	—	—	—	—	—
	long	2.56	1.760	3.957	1.510	2.490	1.060	1.400
$abab +$	short	1.62	1.200	—	—	1.380	—	—
	long	2.22	—	3.200	1.310	—	0.880	1.190
Information-handling rate of subject expressed in Bu/sec.			4.7	2.2	6.3	3.6	8.4	6.1

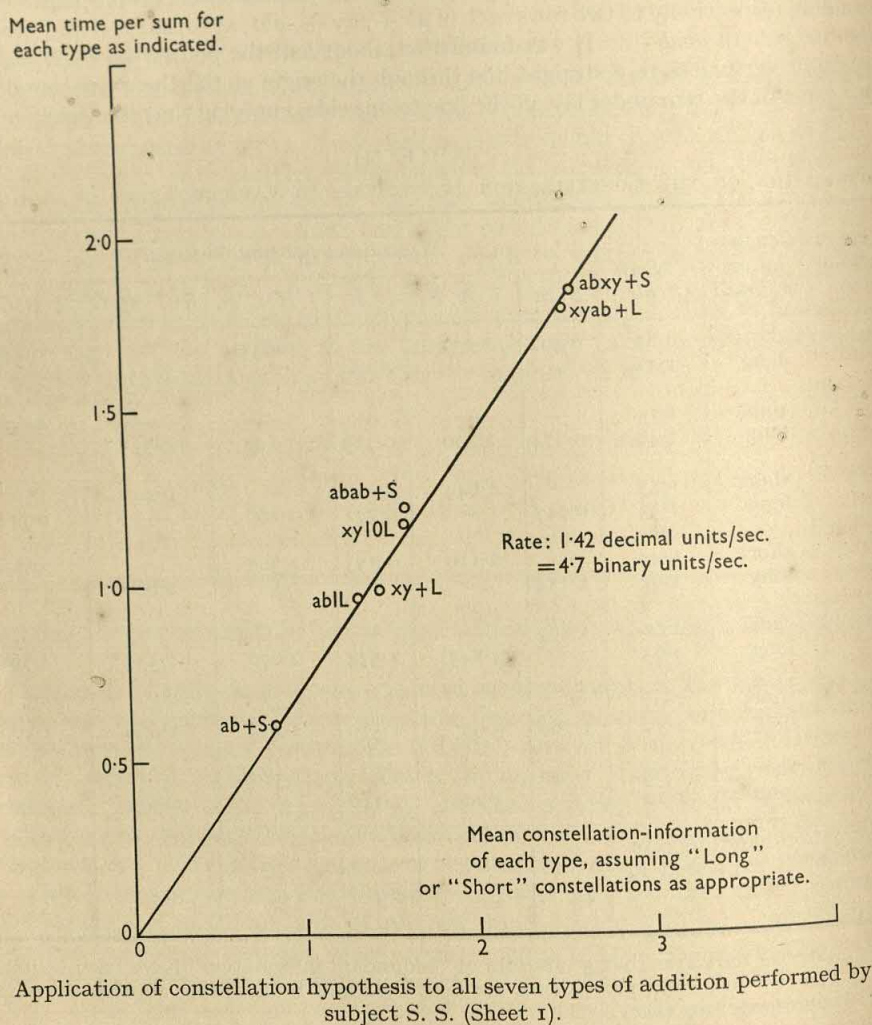
Note: Asterisk indicates that 3 bits/sum of "redundant information" have been assumed. The two results in parentheses are best explained by adopting another subsidiary hypothesis (see text).

was less than predicted. The simplest explanation for these aberrant points, short of rejecting the constellation hypothesis entirely and seeking a new explanation for results such as those of subject B. D., is that subjects can sometimes perform a calculation in such a way that their rate of performance is determined by an abridged constellation, from which some of the numbers required for an exhaustive description of the calculation are omitted, the constellation-information being thereby reduced.

The *third principle* therefore qualifies the first and offers an alternative to it. It contains the subsidiary hypothesis that the time taken by some subjects over some

calculations is determined by a "short" constellation which comprises *only some of the numbers* required for an explicit description of the calculation. It was found in practice that the numbers to be omitted from the "long" constellation-diagrams, to produce "short" diagrams which would fit all, but three of the remaining observations, were as follows.

FIGURE 2



In the case of $ab +$ and $xy +$ the omission of either the problem-digits or the answer gave the desired result, both courses being quantitatively identical. Other experiments on one-stage sums, such as $ab -$ or $xy \times$, in which the answer is not equal to the sum of the problem-digits, suggest that it is the answer which should be omitted and the short $ab +$, $xy +$ diagrams will provisionally be written $[a, b]$ and $[x, y]$. For $ab1$ and $xy10$ sums it is apparently the intermediate sum $(a + b)$ or $(x + y)$ which must be omitted, yielding the short diagrams $(a, b : 1 : (a + b + 1))$ & $[x, y : 10 : (x + y - 10)]$. Finally, it was supposed that short constellation-diagrams for the

two-stage sums $abxy +$, $xyab +$ and $abab +$ might be constructed by combining the appropriate short constellations according to the same *second principle* as before, writing a short $abxy +$ diagram as $[x,y:10:(x + y - 10)].[a,b:1:(a + b + 1)]$ and similarly for the rest.

Applying these short constellation-diagrams to the appropriate rows on Sheet 1, "short" values for $Q[\text{calc.}]$ were obtained in the same way as before and are entered in Table I in the columns headed $+S$, $-S$, $\times S$ and $ab1S$, $xy10S$. Using these "short" constellation-information values where necessary as alternatives to the "long" values, all save three of the 42 graph-points (seven types of calculation by six subjects) were satisfactorily accounted for. The experimental data are given in Table III, where they are entered so as to show, for each case, whether a short or a long constellation had to be assumed in order that all seven points, for each subject, should lie about a single straight line through the origin. The Table also shows the information-rates deduced from the slopes of these lines; they range between 2.2 and 8.4 BU/sec. Figure 2 shows how exact a fit may be obtained with a certain type of subject; but even with an erratic subject (S. R.) the informations corresponding to short and long constellations differ sufficiently for there to be little difficulty in deciding which should be assumed.

One of the three results in Table III which are not explained by the foregoing assumptions is that of R. J. M. G. on $abxy +$ sums. However, if to the long constellation information are added 3 bits/sum of "redundant information" (as for the first $xy \times$ row done by B. D.) a very close fit is obtained. The remaining two anomalous results also concern $abxy +$ calculations, by J. A. F. and H. B. G. T.; but for these a different type of explanation (depending on the notion that some other process than the handling of constellations is rate-limiting) gives a more exact fit than to assume the handling of any whole number of bits/sum of "redundant information."

Further experiments on one-stage calculations

Several additional subjects have recently been submitted to long experiments in which four or more rows are done of each type of one-stage sum, viz. $ab -$, $ab +$, $ab \times$, $xy -$, $xy +$ and $xy \times$. The average time-requirement for each type of calculation, pooling like rows, is accounted for satisfactorily and in some cases very exactly, for every subject, by invoking either a short constellation of the form $[p,q]$ or a long constellation of the form $[p,q:r]$, save that with each subject it is necessary to postulate the handling of "redundant information" with one of the six types of sum. (Table IV).

Apart from their general confirmatory value these experiments raise two points of interest. The first is that from notes taken at the time of each experiment, the type of sum felt subjectively to be most difficult seems to be that for which the handling of "redundant information" has afterwards to be postulated.

Subject G. G. confessed to having great trouble with $xy \times$ sums, saying that he had never been sure of his 7-, 8- and 9-times tables. F. de M. found subtraction the most difficult operation since she was in the habit of transposing the digits when the smaller was on top; she was emphatic that this difficulty affected $ab -$ more than $xy -$ although the reason is obscure. B. T. on the other hand, for whom the short $ab -$ constellation fits perfectly, named subtraction as the easiest operation; large numbers were difficult and this difficulty was especially evident with $xy \times$ calculations; with these sums "you have to think" and it "seems to be slower." With other types of sum, however, "you don't think about the operation but just write down the answer . . . like pressing button A and it pops out without thinking about what goes on in the machinery."

There may therefore be a parallel between the "redundant information" of the constellation hypothesis and the "redundancy" of communication theory, which serves the useful purpose of protecting the message against loss of information in the presence of noise and so enables greater confidence to be placed in the message transmitted. Subjects may handle "redundant information," or allot extra time *pro rata*, where they feel that the risk of error is especially great.

Subjective comments such as those quoted above suggest, secondly, that the adoption of short constellations in some cases may have to do with the *distribution of attention*. When numerical examples of short *ab1* and *xy10* constellations are written:

$$\begin{array}{r} 2 \\ 4 \text{ and: } 7 \\ 1 \\ \hline 7 \end{array} \quad \begin{array}{r} 9 \\ 7 \\ 10 \\ \hline 6 \end{array}$$

it seems intuitively reasonable to suppose that the subject contrives to avoid handling the intermediate sums altogether, perhaps by regarding the conceptual stimuli as $O(a,b,1)$ or $O(x,y,-10)$ respectively. However, this explanation does not account for the short constellations, of the form $[p,q]$, used in cases such as $xy -$ (B. T.), $ab +$ (S. S., J. H. L. B., etc.) or $xy +$ (H. B. G. T.), because it is obvious that both the problem-digits and the

TABLE IV
FURTHER RESULTS ON ONE-STAGE CALCULATIONS

Calculation	Constellation assumed	Mean $Q[calc.]$	F. de M.	B.T.	G. G.
$ab -$	short	0.81	—	—	0.57
	long	0.95	0.66*	0.58	—
$ab +$	short	0.81	—	—	0.57
	long	1.12	0.59	0.64	—
$ab \times$	short	0.81	—	—	—
	long	1.21	0.73	0.73	0.84
$xy -$	short	1.16	—	0.65	0.72
	long	1.24	0.75	—	—
$xy +$	short	1.16	—	—	—
	long	1.46	0.79	0.85	0.94
$xy \times$	short	1.16	—	—	—
	long	1.80	1.03	1.18**	1.79***
Information-handling rate of subject expressed in BU/sec.			5.74	5.70	5.07

Note: Asterisks denote number of bits/sum "redundant information" assumed.

answer must in reality be handled by the brain. A possible explanation is that *no attention is paid* to those parts of a calculation which do not contribute to the constellation information or, therefore, to the relative time-requirement. There is considerable evidence of an anecdotal sort to the effect that phenomenally rapid calculators employ mental processes which they either cannot or do not choose to introspect and which can only be called "unconscious."

The need for the secondary postulates of "redundant information" and "short constellations" is made more evident by the results of the following experiment. It seems that neither of these complications intervenes in the performance of four-stage multiplications of the type $[x \cdot \log x]$, which therefore serve to place the main hypothesis on a more secure basis and so to emphasize the need for an explanation of the anomalous results sometimes obtained on two-stage additions and one-stage calculations of various sorts.

A further generalization of the operator Q

For several reasons it would be satisfactory if the operator Q could be shown to be applicable to other categories of calculation than those so far discussed. The technique of computing the information-requirements of a set of sums, measuring the time taken by the subject to perform them, plotting information against time and deriving the information-handling rate of the subjects from the graph would be greatly simplified and rendered much more reliable if only one type of arithmetical operation could be employed without unduly limiting the range of constellation-informations covered, especially if it transpired that in such sums, subjects invariably used long constellations. The third principle could then be ignored and the standard operator Q would be applicable to all subjects; moreover, certain mathematical applications of the operator would acquire a psychological foundation, with all the implications this would entail.

The type of calculation to be considered now is that in which a number of one digit multiplies a number of two or more digits. We shall refer to the multiplier as X , where X takes values, 2, 3, 4, ... 9, and to the multiplicand as N . If a general solution can be found for this problem, the way will be open for the application of the operator Q to a variety of special cases. However, the definition of Q is insufficiently general, as it stands, to be applied to all calculations of this type and it must be broadened in two respects.

Firstly, the *second principle*, according to which two-stage sums such as $abxy +$ were treated as comprising two constellations handled consecutively, must be extended to cover the case where the calculation comprises a greater number of stages. By analogy, we shall assume that when the multiplicand has D digits and the multiplier has one, the calculation is handled by the brain as a series of D successive constellations. To take a numerical example involving no carrying, the constellation-information of the sum $3.2132 = 6396$, according to this extended second principle, would be: $Q[3.2132] = \log(3 + 2 + 6) + \log(3 + 3 + 9) + \log(3 + 1 + 3) + \log(3 + 2 + 6)$. Numbers printed in heavy type denote answer-digits, as before, and it has been taken that the multiplication proceeds from right to left.

The second extension involves the *first principle*. The experiments up to this point have only required the number 1 to be carried; but in multiplications of the sort under discussion, situations commonly arise where numbers up to 8 have to be carried. It will be recalled that the carried 1 in $abxy +$ sums was dealt with by combining an $xy10$ and an $ab1$ constellation; an analogous procedure is easily devised for other carried numbers. For an $abxy +$ such as $29 + 37$ the constellation diagrams might be written:

2	9
3	7
—	—
5	16
1	10
—	—
6	6

The carrying is represented by including a subtracted 10 in the first of the two constellations and the carried 1 in the second. It will be assumed, by strict analogy, that in multiplications the same method should be used for higher multiples of ten as well as for ten itself; if the number to be carried is 8, for example, then 80 will be subtracted from the intermediate product, just as 10 was subtracted from the

intermediate sum above; and in the next constellation, 8 will be added to the intermediate product. The multiplication $9.29 = 261$ will illustrate the point:

$$\begin{array}{r}
 2 \quad 9 \\
 9 \quad 9 \\
 \hline
 18 \quad 81 \\
 8 \quad 80 \\
 \hline
 26 \quad 1
 \end{array}$$

Where the multiplicand has more than two digits, it often happens that the sum of the intermediate product and a number carried forward to that stage is greater than 10, so that a number has to be carried to the next stage. In this event, the carrying can still be dealt with in the same way:

$$\begin{array}{r}
 3 \quad 2 \quad 1 \quad 9 \quad 7 \\
 6 \quad 6 \quad 6 \quad 6 \quad 6 \\
 \hline
 18 \quad 12 \quad 6 \quad 54 \quad 42 \\
 1 \quad 1 \quad 5 \quad 4 \quad 40 \\
 \hline
 19 \quad 13 \quad 11 \quad 58 \quad 2 \\
 \quad 10 \quad 10 \quad 50 \\
 \hline
 3 \quad 1 \quad 8
 \end{array}$$

In all these examples, it may plausibly be argued that the first principle has been strictly applied; one is not usually aware of performing all the steps which are explicitly represented in these constellation-diagrams, yet *they are implied*.

Two special cases occasionally arise and these have been dealt with arbitrarily in ways which seemed reasonable. The first is where the multiplicand contains a zero and the second is where the intermediate product happens to be a multiple of ten. The procedure adopted is illustrated in the following two examples:

$$\begin{array}{r}
 2 \quad 0 \quad 4 \quad 6 \quad 7 \quad 4 \quad 1 \\
 9 \quad 9 \quad 9 \quad 5 \quad 5 \quad 5 \quad 5 \\
 \hline
 18 \quad 0 \quad 36 \quad 30 \quad 35 \quad 20 \quad 5 \\
 \quad 3 \quad 30 \quad 3 \quad 2 \quad \quad \quad \\
 \quad \quad \quad 6 \quad 33 \quad 37 \quad 30 \quad \quad \quad \\
 \quad \quad \quad \quad \quad \quad 7
 \end{array}$$

However, the precise rules followed in these occasional special cases are often not as important as might be imagined, since the total constellation-information, $\log(Z_1.Z_2.Z_3 \dots Z_D)$, is relatively insensitive to changes in one of the Z , in most cases.

We are now ready to apply the operator Q , in its generalized form, to the family of multiplications of the form $[x.\log x]$, where x takes the values 2, 3, . . . 9, and the multiplicand is the 4-figure logarithm of x to base 10, the decimal point being ignored. This particular family of multiplications has been chosen because $Q[x.\log x]$, over

TABLE V

* CONSTELLATION-ANALYSIS OF MULTIPLICATIONS OF THE TYPE $[x \cdot \log x]$

x	Analysis				Values of Z	$Q[x, \log x]$	$\sum_2^x Q[x, \log x]$	$x \cdot \log x$
2	3 2 — 6	0 2 — 0	1 2 — 2	0 2 — 0	2 5 2 11	2.3424	2.342	0.602
3	4 3 — 12 2 — 14	7 3 — 21 2 — 23 20 — 3	7 3 — 21 20 — 1	1 3 — 3 — — —	7 52 79 35	6.0028	8.345	1.431
4	6 4 — 24	0 4 — 0	2 4 — 8	1 4 — 4	9 14 4 34	4.2339	12.579	2.408
5	6 5 — 30 4 — 34	9 5 — 45 4 — 49 40 — 9	9 5 — 45 40 — 5	0 5 — 0 — — —	5 104 161 79	6.8204	19.399	3.495
6	7 6 — 42 4 — 46	7 6 — 42 4 — 46 40 — 6	8 6 — 48 1 — 49 40 — 9	2 6 — 12 10 — 2	32 161 151 105	7.9121	27.312	4.669
7	8 7 — 56 3 — 59	4 7 — 28 3 — 31 30 — 1	5 7 — 35 30 — 5	1 7 — 7 — — —	15 82 104 133	7.2308	34.542	5.916
8	9 8 — 72	0 8 — 0 2	3 8 — 24 20 — 4	1 8 — 8 — — —	17 59 10 89	5.9507	40.493	7.225
9	9 9 — 81 4 — 85	5 9 — 45 3 — 48 40 — 8	4 9 — 36 1 — 37 30 — 7	2 9 — 18 10 — 8	47 124 158 188	8.2384	48.732	8.588

the range $2 \leq x \leq 9$, has been found empirically to have a curious and useful property.

In Table V is shown the analysis of each of the eight possible calculations of this type, according to the principles defined above. Each is split into four constellations, one for each digit of $\log x$. In the next column, the sums of the numbers in each constellation (Z_1, Z_2, Z_3 & Z_4) are listed. In the next is given, for each calculation, the sum of the logarithms of these Z . This quantity is $Q[x.\log x]$ and the constellation hypothesis predicts that for a given subject, the time t_x taken to perform the calculation $[x.\log x]$ should be proportional to $Q[x.\log x]$, since the latter is supposed to give the amount of information which must be handled to perform the calculation. In formal terms, we expect:

$$k.t_x = Q[x.\log x] \quad \dots \quad (i)$$

In the next column of the Table the cumulative sum of $Q[x.\log x]$ has been entered. Opposite each value of x is given the value of $Q[2.\log 2] + Q[3.\log 3] + \dots + Q[x.\log x]$, which is *ex hypothesi* the total amount of information which would be handled by a subject who performed in turn the calculations $[2.\log 2], [3.\log 3], \dots, [x.\log x]$. If we represent the times taken over each of these, separately timed, as $t_2, t_3, \dots, t_x, \dots, t_9$, the theory predicts that the sum of these times up to and including t_x will be strictly proportional to the corresponding cumulative sum of $Q[x.\log x]$, a prediction we may write:

$$k.\sum_2^x (t_x) = \sum_2^x Q[x.\log x] \quad \dots \quad (ii)$$

—where k as before is the information-handling rate.

The advantage of using this prediction (ii) rather than (i) as a basis for experimental test is that since each of the calculations is short the timing error may be relatively large; the subject has no opportunity to settle into his stride and variations in the reaction-time of the examiner may no longer be negligible. However, some of these variations are likely to be random so that the summation of successive observations should tend to cancel them out.

Now it so happens that if $\sum_2^x Q[x.\log x]$ is plotted against $(x \log x)$ itself, the points lie very closely about a straight line passing through the origin, of the form:

$$\sum_2^x Q[x.\log x] = 5.65 \, x.\log x \quad \dots \quad (iii)$$

This curious finding is only one of several similar relationships which will be described elsewhere and which might appropriately be called meta-arithmetical propositions since they concern properties of the arithmetical language. The present importance of equation (iii) lies in the fact that it can be substituted into the prediction (ii) to give:

$$k.\sum_2^x (t_x) = 5.65 \, (x.\log x) \quad \dots \quad (iv)$$

The prediction to be tested is now the following. If the subject is made to do the calculations $[2.\log 2], [3.\log 3], \dots, [x.\log x] \dots [9.\log 9]$ (in any order) and the corresponding times $t_2, t_3, \dots, t_x, \dots, t_9$ are recorded, then if $\sum_2^x (t_x)$ is plotted against $(x.\log x)$ the points will lie on a straight line through the origin. The slope of this line, multiplied by 5.65, will yield the information-handling rate of the subject in DU/sec. The value of the constant 5.65 depends, of course, on the number of digits to which $\log x$ is taken in the multiplicands.

This prediction assumes the fundamental axiom that in calculation the brain behaves as a communications device for which the time-requirement of a task is

strictly proportional to its information-requirement. It also includes the assumption that—in this family of multiplications at least—the information-requirement of a calculation can be computed in a standard manner for all subjects, given the numerical character of the calculation and the rules of arithmetical procedure. As we have seen, the second assumption is only sometimes true, as it stands, for simple additions and one-stage sums in general; for these, subsidiary hypotheses are required. However, the foregoing prediction seems to be consistently borne out experimentally and no subsidiary hypotheses are needed. It may be that with the more complex calculations, the tendency is for normal subjects to show less, not more, diversity of function and to follow the strict ritual of calculation more slavishly.

In these experiments, subjects were "warmed up" partially with one row of $abxy +$ and one of $xy \times$. The calculations were then written down one by one on a blank page before the subject, whose performance on each one was timed before the next was written. The sums were presented in the order

3010	4771	6021
2	3	4, etc.,

but if the subject broke down on one of them, it would be presented again later, out of turn. Mistakes were ignored but the usual instructions were given.

In every subject so far tested, there was no difficulty in drawing a straight line through the origin and the eight points. The scatter was only pronounced in one case (B. M.), who made gross mistakes in five of the sums and was apparently incapable of "carrying." At least three other subjects (G. G., J. A., S. B.) said beforehand that they were uncertain of their 7-, 8- or 9-times tables. The fit is generally good and in

TABLE VI

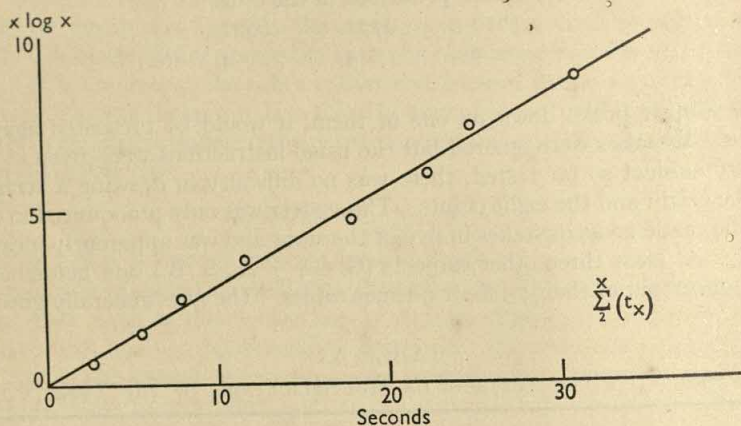
TIMES TAKEN (t_x) BY 12 SUBJECTS ON MULTIPLICATIONS OF THE TYPE $[x \cdot \log x]$

x:	2	3	4	5	6	7	8	9	Rate: BU/sec.
B. T.	1.60	3.90	2.35	3.70	6.30	4.30	2.60	6.00	5.21
G. G.	1.50	4.45	2.80	4.60	6.10	6.35	4.20	6.00	4.52
R. S.	2.10	5.80	3.35	5.50	11.90	8.30	3.05	8.50	3.39
B. M.	1.80	8.00	4.80	10.20	16.10	16.30	7.50	10.55	2.16
A. B.	2.80	4.35	3.55	6.05	8.50	6.40	3.80	8.80	3.65
J. A.	2.45	4.45	4.45	5.30	8.10	6.65	3.65	7.10	3.85
H. B. G. T.	1.50	3.25	2.10	4.15	4.80	3.25	2.20	4.90	6.03
J. B.	2.65	4.25	3.10	5.15	8.20	7.70	4.95	7.50	3.76
S. B.	3.20	8.60	4.95	6.15	10.35	13.00	6.35	13.25	2.54
R. B.	2.40	7.00	3.65	6.25	9.30	6.25	3.00	6.20	3.50
F. de M.	2.30	2.95	2.75	3.55	4.05	4.35	3.45	5.25	5.65
F. L.	3.45	5.35	2.95	4.55	8.10	5.05	4.50	6.15	3.95

one case (F. de M.) it is virtually perfect. The times (t_x) and the information-handling rates deduced, for all twelve of the subjects so far submitted to this experiment, are given in Table VI. Figure 3 illustrates a typical graph.

The rates obtained by this "summation method" tend to be slightly low, where it has been possible to compare them with rates obtained with Sheet 1 or one one-stage sums alone. It may be preferable in future to give subjects more "warming-up" or to take them through the $[x \cdot \log x]$ series twice. Nevertheless, the rates cover a range of 2.16–6.03 BU/sec. and are comparable to the rates of Table III for written answers. Furthermore, in recent experiments (Thomas, 1961) where ten young normal adults produced timed runs of digits written "at random," the first-order symbol-choice information-output rates (C) varied from 2.12 to 5.49 absolute bits/sec., information

FIGURE 3



The summation method applied to subject B. T.

being defined conventionally in terms of the relative frequencies of occurrence of the various symbol-types. These directly measured self-paced *output* rates compare closely with the information-handling rates indirectly inferred by the summation method. Information-handling rates of the same order have been estimated to be required for the perception of musical melodies (Moles, 1958) according to the statistical definition of information. These facts suggest that the information-rates inferred in the present experiments and those based on the statistical definition of information which is current in communication theory, both refer to the same scale of measurement; in other words, that one decimal unit of $Q[\text{calc.}]$ is equivalent to one absolute decimal unit of statistical information.

If the central pillar of the constellation hypothesis—the generalized definition and the meaning of the operator Q —can be taken as established, then the need for secondary hypotheses, such as "short constellations" and "redundant information," to account for atypical results, becomes apparent. These and other psychological implications of the constellation hypothesis; its neurophysiological aspect; its further testing and the further generalization of Q ; the mathematical results of applying Q to families or series of multiplications other than $[x \cdot \log x]$ and the integration of the constellation hypothesis with the hypothesis of the rate-limiting mental process (Thomas, 1960a, b) are all topics on which some progress has been made and which it is hoped to pursue in due course.

My sincere thanks are due to Prof. F. Lhermitte, of the Centre de Langage, Hôpital-Hospice de la Salpêtrière, for every kind of assistance; and to those friends, colleagues and students who submitted themselves to these experiments. The more recent part of the work was helped by an M.R.C. Exchange Scholarship.

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DETECTION OF VISUAL PATTERNS DISTURBED BY NOISE: An Exploratory Study

BY

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An introductory study of the perception of stochastically specified events is reported. The initial problem was to determine whether the perceiver can split visual input data of this kind into random and determined components. The inability of subjects to do so with the stimulus material used (a filmlike sequence of dot patterns), led to the more general question of how subjects code this kind of visual material. To meet the difficulty of defining the subjects' responses, two experiments were designed. In both, patterns were presented as a rapid sequence of dots on a screen. The patterns were more or less disturbed by "noise," i.e. the dots did not appear exactly at their proper places. In the first experiment the response was a rating on a semantic scale, in the second an identification from among a set of alternative patterns. The results of these experiments give some insight in the coding systems adopted by the subjects. First, noise appears to be detrimental to pattern recognition, especially to patterns with little spread. Second, this shows connections with the factors obtained from analysis of the semantic ratings, e.g. easily disturbed patterns show a large drop in the semantic regularity factor, when only a little noise is added.

INTRODUCTION

In recent years several attempts have been made to construct theoretical models for aspects of the visual perception process. Some of them depend on statistical reasoning; it is assumed that it is possible for the perceiver to split input data into random and determined components, or, what amounts to the same thing, to detect the parameters which specify a stochastic process. An example of this kind of model is Rosenblatt's "Perceptron." (1958.)

Statistical separability in this sense is the subject of the experiments described in the present report. They deal with separability on the "macro" level. The consideration that they may have some bearing on separability processes in neural networks is not precluded, but this side of the matter is not our present concern. We shall investigate how far the human observer is able to keep random and determined components apart in stimulus material.

The idea behind the theory of separability is clear enough. If the input rate exceeds the channel capacity, then the organism is forced to reject data and it would be an efficient procedure to disregard random components in favour of more stable and enduring characteristics of environmental events. Even below the channel capacity perceptual load would be reduced and efficiency increased if it were possible for the organism to extract the constant components in a given mixture of determined and stochastic events. Such a procedure would minimize the "surprise" value of the stimuli and this is identical with minimizing perceptual load.

In order to follow the argument it may be helpful if the reader visualizes the kind of stimulus material we are considering and which was used in the experiments to be described. Suppose dot patterns are given in a rapid succession of presentations, as could be achieved by using cine-film. A particular pattern, say a square of four dots, could be presented and it could then be "disturbed" by added noise. The location of each corner of the square might for instance be specified by a bivariate normal distribution. The mean of this distribution would coincide with the correct position of

the corner of the square, but in any particular presentation the dot would depart somewhat from this exact position, to an extent dependent on the variance of the distribution. Thus a succession of patterns looking like distorted squares would be presented.

Are subjects able to recognize the square in such a sequence of distorted presentations? If they succeeded, the surprise value of the later stimuli in the sequence would be minimized. Perception would then show efficient coding in the sense used by Attneave (1954). Instead of perceiving a whole series of different patterns the subject would see a steady pattern as a first component, and the second component would be seen as a "quivering" of the first.

This kind of perceptual task is found in many real cases, for instance in radars, where the operator has to identify the track of the moving object from a succession of signals on the screen. Here some noise is practically always to be expected; noise resulting from the working of the apparatus itself, or from the fact that the scanning beam picks up an echo from different parts of the object in successive rotations. An object moving along a straight line appears on the screen as a zigzagging of discrete dots.

There are a number of problems to be solved in dealing with this kind of stimulus material.

(1) The first problem is the difficulty of defining the subject's *response*. People appear to perceive this sort of stimulus material in a variety of ways, and if they are left to describe freely "what they see," such descriptions are so divergent that they cannot be used to measure or test anything. There are two possible solutions, to have subjects learn a particular kind of response or to restrict the range of free responses. In the first case one attempts to have the subject attend to a particular aspect of the stimulus material, in the second kind one forces him to produce a particular kind of response. In both procedures the original "richness" of perception will be to some extent hidden from the experimenter. Careful questioning could, of course, reveal anything we might be interested in but it is felt that in the present experiments the difficulty of response specification is far from being resolved.

(2) The second problem concerns the nature of the *coding system* used by the subject. It is clear that our perception of a rapid sequence of random patterns can only be in rather global terms. The sequence of stimuli is certainly not wholly reproducible and it follows that ones which are actually different in all their details, might appear very similar to the subject. This indicates that the subject is performing some kind of coding: sequences which are seen as similar, or where the subject cannot specify the difference or where he perceives them as instances of the same general prototype, are coded in the same way. What is the most effective form of coding, and what is it the subject picks out of this kind of stimulus material?

(3) The third problem is the one mentioned above: are subjects able to separate stochastic and fixed components? Are they able to recognize the parameters behind the stochastic production of patterns?

THE EXPERIMENTS

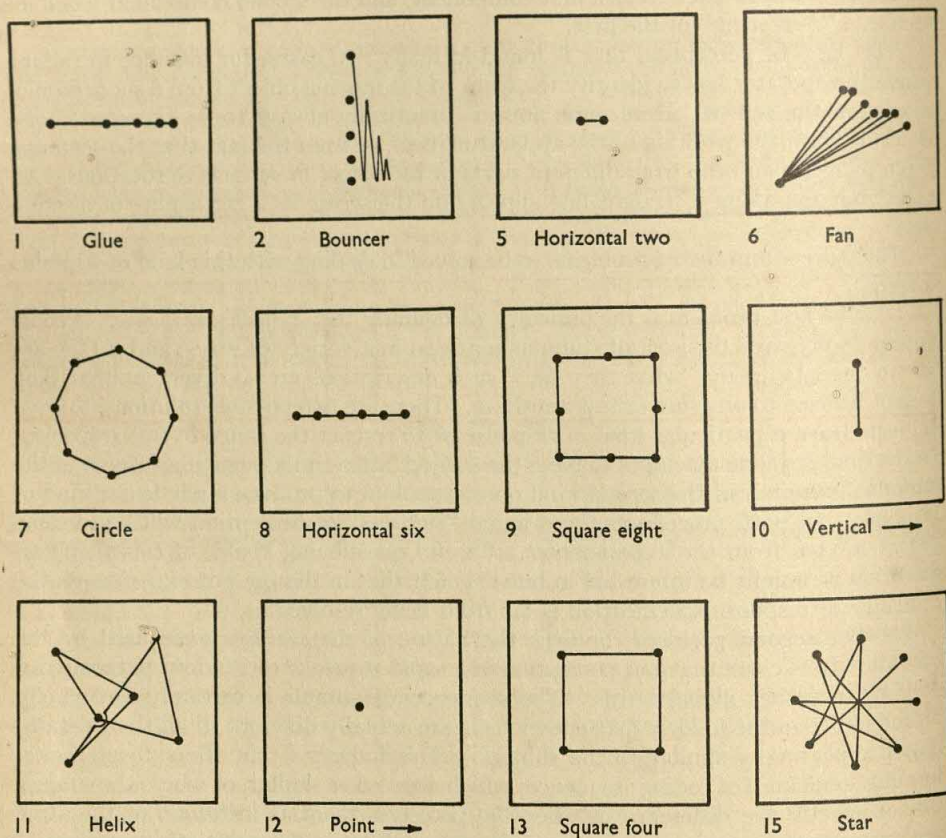
A number of preliminary experiments showed that subjects have extreme difficulty in detecting characteristics of stimulus sequences when these characteristics are masked by noise, in the sense described above. Speaking in statistical terms one may say that human perceivers are extremely prone to type II errors: they maintain the null-hypothesis even if there is abundant evidence against it according to statistical criteria. This negative result was partly responsible for the design of the following main experiments. It was decided to use a recognition method, in order to give the subjects the best chance of revealing any capacities they might have after all—and second, Osgood's semantic

scaling technique was chosen for the same reason, and also to gather some hints as to which categories might be effective in perception.

MATERIAL

The experimental material consisted of 41 film strips each with 40 opaque frames. Each frame contained one hole, located somewhere in an 18×18 matrix. (The holes were made in the film by punching through selected holes in a metal template.) The strips were

FIGURE 1



The 12 patterns used in the semantic rating and the pattern-recognition experiments. Pattern 3 was the Brownian variant of 10 ("Brownian dance"), pattern 4 was the Brownian variant of 12 ("Brownian movement") and 14 was the bivariate rectangular density distribution.

shown by means of a slowed-down cine projector at a rate chosen to facilitate the appearance of stroboscopic movement. The white spot projected on to a 90×90 cm. screen thus moved to a series of positions describing a chosen pattern, such as a square, horizontal straight line, or circle.

Fourteen patterns were used of which 12 are shown in Figure 1, the numbers at each position indicating the order of presentation. The patterns were given arbitrary names, as shown above.

The patterns could be shown either in pure or noisy form. Noise was introduced by displacing each dot a random distance from its proper position in the matrix. Horizontal and vertical displacements were random normal deviates with zero mean and variance

1.64 (Level 1) or 3.03 (Level 2) matrix units. The effect of the "noise" was that the patterns shown in Figure 1 appeared on the screen either less or more distorted.

The use of 12 patterns at three noise levels (zero, 1 and 2) accounts for 36 strips. Noise can also be added in a different way, by locating the mean of the bivariate distribution at the last position of the dot instead of at a fixed position. This procedure was applied to patterns 12 and 10, resulting in a kind of Brownian motion of a single dot, and in a Brownian dance of two dots respectively. The previous two levels of variance were used, giving 4 strips. The series of 41 was completed by presenting a single dot located at random in bivariate rectangular distribution.

A third experimental variable was rate of presentation; fast or slow, corresponding to 2 and 1 presentations per sec. respectively. To economise in time rate was not varied for all the patterns. The final arrangement is shown in Table I, the numbers in the cells referring to the order of presentation of the 41 series in the recognition experiment.

TABLE I
THE ORDER OF PRESENTATION OF THE 41 FILMSTRIPS
This order was reversed for the second group in experiment II

Pattern	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Noise Level 2	16	2	29	19	30	10	25	12	11	33	13	37	38		39
" 1	4	6	20	17	23	31	41	24	21	28	8	26	14	34	35
" 0	9	36			1	15	7	18	32	40	3	5	22		27



slow



fast

SEMANTIC RATING EXPERIMENT I

Procedure

Subjects were given a list of 19 semantic scales of the Osgood type, each with 7 positions. They are given in Table II, except for Scale 12 (centrifugal-centripetal) which was discarded for lack of discrimination.

The subjects were 31 psychology undergraduates in the University of Leiden. They were divided into two groups and each group had to rate the films on 10 scales (Scale 3 was made common to both in order to have some check on inter-group reliability; a correlation of $r = 0.94$ was obtained).

Results

The ratings were factor-analysed by the centroid method and the results are given in Table II. The factors were rotated to new orthogonal positions according to a cluster inspection (no simple structure was evident, and it is not theoretically expected since the scales are not supposed to represent an unbiased sample from the universe of categories). The loadings on the rotated factors are also given in Table II.

The factors may be tentatively labelled on the basis of the loadings as follows:—

factor I—(heavy, slow, dull, passive)—“rate”;

factor II—(determined, even, symmetrical, stable)—“regularity”;

factor III—(closed, beautiful, round)—“gestalt.”

Inspection of the strips of patterns with high or low factor scores revealed that factor I discriminates between the slow films and the fast ones with no overlap and it thus appears to have a clear denotative meaning.

Factor II appears to distinguish between noiseless and noisy patterns. Table III gives factor score estimates. The noiseless patterns scored significantly higher than the noisy ones ($p < 0.01$) and the patterns with low-level noise scored higher than those with high-level noise ($p < 0.05$). Therefore factor II also has a clear denotation.

Factor III has some connection with noise but not at a significant level. The scales which are highly loaded on this factor apparently refer to characteristics of the

TABLE II
(a) FACTOR LOADINGS OF SCALES BEFORE AND AFTER ROTATION

Scales	Raw loadings			h^2	After rotation		
	I	II	III		Factor I "rate"	Factor II "regu- larity"	Factor III "gestalt"
1. Closed—Open ..	428	561	-406	663	-183	451	650
2. Heavy—Light ..	757	-574	-152	926	884	371	082
3. Evenly—Varying ..	890	228	289	928	171	948	-016
4. Beautiful—Ugly ..	419	671	-182	659	-330	562	481
5. Determined—Random	841	521	033	980	-046	936	312
6. Passive—Active ..	874	-430	020	949	781	585	-003
7. Round—Angular ..	493	051	-355	372	270	323	437
8. Slow—Fast ..	709	-567	-253	888	877	299	168
9. Is displaced—Moves itself ..	671	-146	-170	500	483	460	232
10. Dull—Vivid ..	879	-439	-056	968	807	561	065
11. Poor—Rich ..	712	-268	267	650	512	593	-203
13. Stable—Labile ..	837	280	336	892	091	939	-051
14. Symmetrical—Asym- metrical ..	815	529	-069	949	-043	883	403
15. Rigid—Resilient ..	754	-275	350	767	520	654	-274
16. Easy to follow— Difficult to follow ..	834	248	-107	769	213	776	343
17. Tied—Free ..	918	219	207	934	210	941	061
18. Always the same point— More different points ..	821	178	254	770	188	858	-015
19. Weak—Strong ..	375	-774	071	745	822	040	-265

(b) ROTATION MATRIX

	I	II	III
I	480	856	190
II	-850	394	350
III	-217	335	-917

patterns themselves, and it is found that the patterns with high estimates in factor III are more "complex" and have more "spread"; they include spots at more widely spaced positions in the matrix—as against ones at only a few or more closely spaced positions.

PATTERN RECOGNITION—EXPERIMENT II

Procedure

The strips were shown in the same way as in the previous experiment, but the rate was kept constant at two presentations per second. Subjects were instructed to identify each pattern from 15 alternatives, which were presented as in Figure 1 (unnumbered) together with the Brownian motion, dance, and rectangular random motion. Care was taken to explain the meaning of the pictures, and the nature of "disturbance by noise." That subjects understood the instructions and used the pictures properly, was apparent from

the fact that they recognized the noiseless patterns correctly with two exceptions to be explained later. (French (1954) used an interesting technique to overcome the difficulty of instructions.)

Subjects were allowed to choose more than one pattern if they felt that they could not decide among them. The number of responses per strip was therefore not constant.

The 24 undergraduates appeared to enjoy the task, in striking contrast to the impression gained in the semantic rating experiment. They were tested in two group sessions, the order of presentation being balanced by reversing the order given in Table I for the second group.

TABLE III
FACTOR SCORE ESTIMATES AND THE DROP IN FACTOR II
ON ADDITION OF NOISE

Noise Pattern	Pattern scores on factor II (in standard scores)*			Pattern scores on factor III (in standard scores)**			Drop in factor II when noise is added	
	0.00 (0)	1.64 (1)	3.03 (2)	0.00 (0)	1.64 (1)	3.03 (2)	(0) - (1)	(0) - (2)
1	1.44	-0.49	-0.85	-0.12	0.37	-0.21	1.39	2.29
2	0.61	-0.87	-1.11	0.16	-0.81	-0.90	1.48	1.72
3	—	-0.08	-0.75	—	-1.04	-0.46	—	—
4	—	-0.79	-1.09	—	-0.37	-0.51	—	—
5*	1.55	-0.56	-0.72	-0.81	-1.39	-0.56	2.11	2.22
6	0.96	-0.11	0.00	-1.58	-1.07	-0.51	1.07	0.96
7	1.44	0.71	-0.03	2.44	1.72	1.95	0.73	1.47
8	1.76	-0.90	-1.27	-0.63	0.16	-0.44	2.66	3.03
9	1.31	0.21	-0.39	1.51	2.67	1.55	1.10	1.70
10	1.88	-0.16	-0.04	-0.19	-0.93	0.09	2.04	1.92
11	-0.34	-1.20	-1.20	-0.49	0.39	-0.19	0.86	0.86
12	1.69	-1.10	-0.86	-0.77	-0.35	-0.74	2.79	2.55
13	1.76	0.87	-0.72	0.35	0.88	0.21	0.89	1.04
14	—	-0.159	—	—	-0.65	—	—	—
15	0.52	-0.18	-0.79	1.18	-0.23	-0.51	0.70	1.31

* Factor II—scores are obtained by adding scores on scales, 5, 13 and 14.

** Factor III—scores are obtained by adding scores on scales 1 and 7, and subtracting score on scale 15.

Results

The raw results (given in Table IV) are not very revealing, and we have devised a measure of stimulus "confusion"—

$$c_{pq} = \sum f_{ip} \cdot f_{iq} / (\sum f_{ip}^2 \cdot \sum f_{iq}^2)^{1/2}$$

where c_{pq} is the "confusion" between stimuli p and q , and f_{ip} is the frequency of choice of alternative i when stimulus p was presented.

The measure can be visualized as follows. A response distribution can be represented as a point in a 15-dimensional space, the coordinate in each dimension representing the frequency of choice of one response alternative, and c_{pq} represents the cosine of the angle between the vectors for stimuli p and q in response space. It is a kind of correlation measure, and the matrix of confusion values may suggest the presence of "factors" in response space.

The measure is not statistically sound, since the sampling distribution is unknown. However, chi-square applied to cell frequencies would fail since the cell entries are often very small and the exact probability approach would have been too time-consuming. Apart from the unknown sampling distribution, the measure disregards

TABLE IV

RESPONSES GIVEN ON PRESENTATION OF VARIOUS PATTERNS

Pattern presented	Noise	2	10	3	1	4	5	8	12	6	7	9	11	13	14	15
2	0 1 2	79 28 6	3	5 13 22		1 6				2 2		1 12			5 10	3
10	0 1 2	4 13 11	87 3	3 34 35		3 34 35					2 2	2	3 4		2 2	
3	1 2	20 12		33 17	5	3 3							8		4 15	2
1	0 1 2	4 1		8 17	35 22 12	13 6		54 2 1	3	1 1	1	4 1 2	2 1		8 2	
4	1 2	7 12		8 5	3	19 18		2		2	2			1	3 6	
5	0 1 2			6 11	4 7 4	13 19	93 1 1		1	6 5			1		1 3	2
8	0 1 2	2 4		11 10	8 15 12	8 20		80 5		2 1	1 3				2 7	1
12	0 1 2	6 1	1	1 19 8	2 14	15 21	1	1 2	88	3		1	1 2	1	1 3	1
6	0 1 2	2 3 2	5 1	4 1 2	3	1				71 35 49	3		1 1		2	6 6
7	0 1 2										91 55 10	6 6			3 20	4 5 10
9	0 1 2			2		2	1	2	4	8 2	28 7	84 7 4	6 11		9 13	7 10
11	0 1 2	1	3			1				2		4 4	48 47 38	1 1	11 11 15	1 7 5
13	0 1 2		3			3					2 10	9 9	10 9	95 34 22		3
14				2		2						1	14		48	
15	0 1 2	1				1					2	7 2	8 6		4 8 18	75 47 27

Subjects were allowed to indicate as many patterns as they liked. Moreover they stated the judged degree of similarity to the pattern on a scale—

- 4 exactly this pattern,
- 3 this pattern, but not exactly,
- 2 it looks like this pattern,
- 1 maybe it is this pattern.

The cell values are sums of these confidence ratings given by all subjects.

TABLE V

CALCULATED CONFUSION BETWEEN PATTERNS (CLUSTERS INDICATED)

(a) Confusion between noiseless patterns (including pattern 14)

	10	I	5	8	12	c 6	7	9	II	13	15	14
2	08					04			01			01
10						e	02		01		02	01
1			02	88				06		e		
5												e
8												
12												
6								10	01	e e	e	
7												07
9									08		01	10
11										e	49	02
13												05
15			e	e							e	

(b) Confusion between patterns with noise level 1.

[illegible]

(c) Confusion between patterns with noise level 2.

[illegible]

the dimensionality of response space so that the real dimensions are not equally weighted.

The calculated values of c_{pq} are given in Table V which shows that noiseless patterns are well recognized ($c_{pq} = 0$ if there is no overlap between the response distributions; $c_{pq} = 1$ if they are identical). The two exceptions are readily explained: subjects take pattern 1 as the random variant of 8, 11 as an instance of 14. Errors of this kind can be easily prevented if noiseless and noisy patterns are not intermingled in the same series.

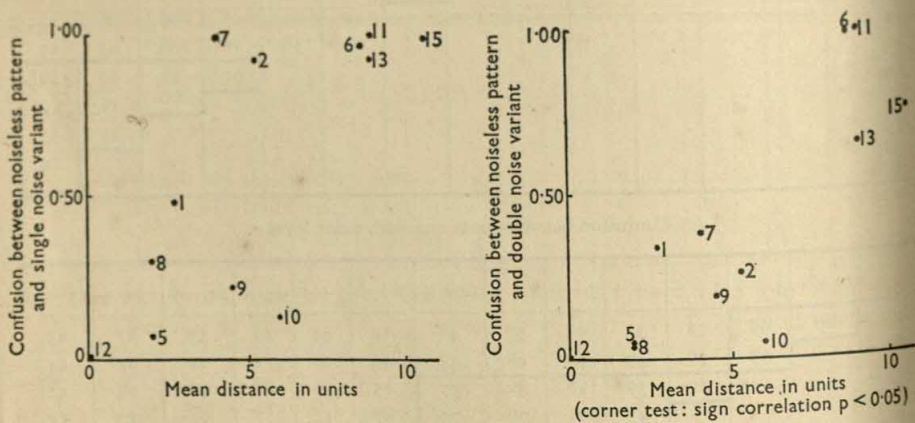
In order to examine the degree to which different patterns resist noise, one should inspect the diagonal cell entries, which are given in Table VI.

TABLE VI
CONFUSION BETWEEN PATTERNS SHOWN NOISELESS AND WITH
NOISE AT LEVEL 1 OR LEVEL 2

	1	2	5	6	7	8	9	10	11	12	13	15
Noise level—1 ..	48	92	07	96	99	30	22	13	99	01	92	98
Noise level—2 ..	34	26	05	99	38	04	19	05	99	00	75	84

Ideally a noiseless pattern should be entirely confused with its noisy variants. High values in Table VI therefore indicate that the pattern is relatively noiseproof and low values the reverse. We see, that 6 (fan), 11 (helix), 13 (square four), and 15 (star) are relatively noiseproof, whereas 1 (glue), 5 (horizontal two), 8 (horizontal

FIGURE 2



Mean intrapattern distance explains the vulnerability of patterns to noise (data of Table V).

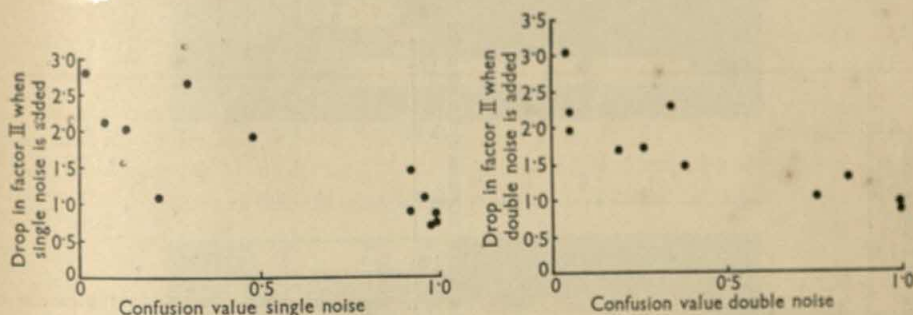
six), 9 (square eight), 10 (vertical), 12 (point) are vulnerable. The explanation is simple: in the noiseproof patterns the average distance travelled by the dot between successive frames is high (8.7, 9.0, 9.0, 10.7 units, respectively), whereas in the vulnerable patterns it is small (2.75, 2.0, 2.0, 4.5, 6.0, and 0.0). Two patterns, 2 (bouncer) and 7 (circle), resist a low level of noise, but are vulnerable to a high level; the mean intrapattern distance here is 5.3 and 4.1 units. Figure 2 shows this result graphically.

On inspection the confusion matrices reveal the presence of several clusters, (1) patterns, 1, 3, 4, 5, 8 and 12; (2) patterns, 2, 3 and 10; and (3) 7, 9, 11, 13, 14 and 15; (4) pattern 6 appears to stand alone.

These "confusion groups" reveal the kind of coding subjects use. The common feature within each group gives an indication of the idea of pattern similarity the subjects might have. We find that group (1) is characterized by horizontal patterns (glue, Brownian dance, Brownian movement, horizontal 2, horizontal 6, point), group (2) by vertical patterns (bouncer, Brownian dance, vertical) and group (3) by extension or spread of pattern (circle, square 8, helix, square 4, star). (4) Pattern 6 (fan) does not clearly belong to any of these groups; pattern 3 (Brownian dance) is mainly a vertical pattern, but the film creates an impression of horizontal motion also. Groups (1) and (2) are actually linked whereas group (3) is separate.

One may conclude that subjects "regress" to very general spatial orientation when identifying noisy patterns. It may once again be stressed that the noisy patterns are

FIGURE 3



Noise resistant patterns (Table V) show smaller drop in factor II if noise is added (Table III), than do vulnerable patterns.

easily discriminable by an ideal detector; since subjects fall so far behind this ideal detector, it shows that they are unable to apply Bayes' theorem in an efficient way.

Discussion of combined results

(1) Since rate of presentation was not varied in the recognition experiment, and since semantic factor I was connected with rate, this factor can be left out of further consideration.

(2) Factor II appeared to refer to "noisiness" of the strips. Table VI indicates which patterns are noise-resistant, and this measure should be related in some way to semantic factor II. Specifically, noise-resistant patterns should show a smaller drop in factor II if noise is added, than vulnerable patterns.

This expectation is borne out, according to Figure 3 where the drop in factor II (Table III) is plotted against the confusion value from Table VI. A simple corner test shows that the relation is significant at the 1 per cent. level. There is another way to show the relation between factor II and confusion. The mean of all its confusion values in the matrix (Table VII) is an index of the general confusability of a given stimulus. If these means (via z - transformation) are taken for the levels of matrix and plotted against the drop in factor II, we again find a significant relationship; for noise level 1 the relation is as good as one might wish (Fig. 4).

TABLE VII

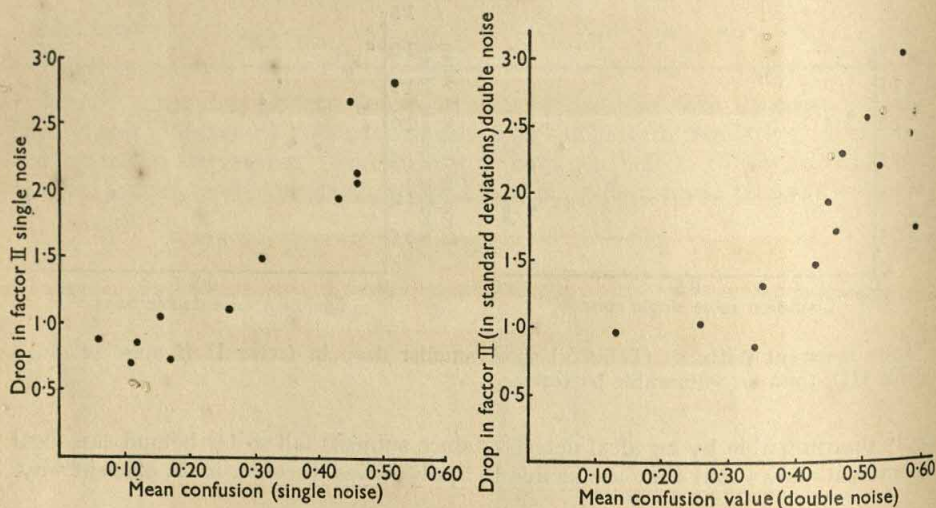
MEAN CONFUSION VALUES (GENERAL CONFUSABILITY)

	1	2	3	4	5	6	7	8	9	10	11	12	13	15
Noise level—1*	43	31	39	47	46	16	17	45	26	46	12	52	06	11
Noise level—2	47	59	55	44	53	13	43	57	46	45	34	51	26	35

The conclusion, then, is that confusion between noisy patterns is mainly a matter of loss of regularity in the pattern. If patterns become undetermined, wild, asymmetrical, and unstable, they tend to be confused with other patterns of this kind.

(3) They tend to be confused, that is, according to very global spatial characteristics. Are these confusion groups related to factor III? This factor was tentatively

FIGURE 4



General confusability of a pattern (mean confusion values—Table VI) is highly related to drop in factor II if noise is added (Table III).

labelled gestalt or complexity factor—it did not only refer to form qualities but also to spread of the fixed position over the matrix. We see from Figure 5 that a relationship is indicated. The patterns at one extreme of factor III form one confusion group, those at the other end make up the two other groups. This difference is significant even for each noise level taken apart. Pattern 6 is a case by itself again.

General conclusions

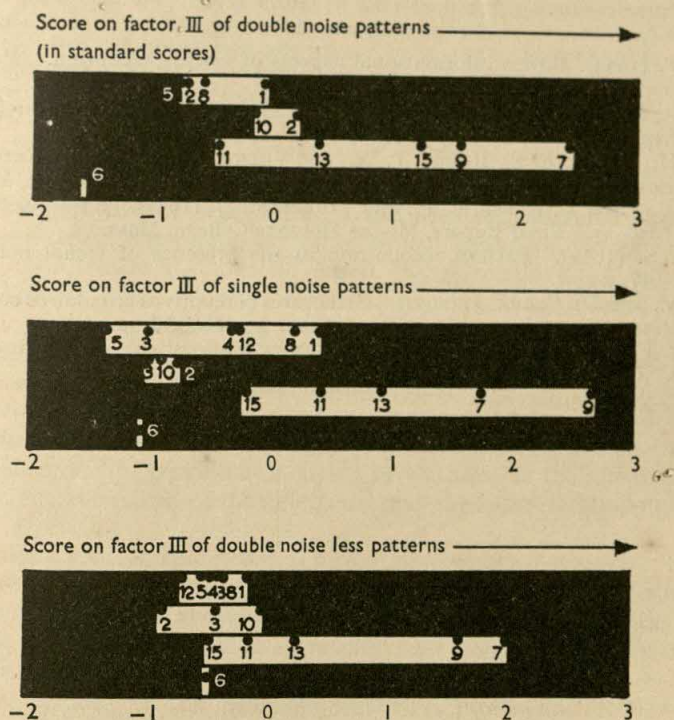
The general conclusion from both experiments, is that human perceivers have a very high tendency to make type II errors, if they have to detect a visual pattern disturbed by noise. Even a moderate amount of noise (moderate in the sense that an ideal detector could easily and nearly faultlessly make correct decision) is detrimental to the perception of temporally presented patterns. The human organism has only a very limited capacity to separate stochastic components from the fixed canvas to

which they are attached. A similar conclusion arises from the work by Corbin, *et al.* (1956, 1958) on detection or convergent grouping in display of dots.

We found that confusion between patterns is mainly a matter of enhanced irregularity. Stable and even patterns are more noiseproof than irregular and undetermined patterns. Patterns which are most affected by noise in this respect tend to be the more easily confused. Patterns appear to fall into groups in that any pattern is mainly confused with alternatives belonging to the same group. These confusion groups appear to arise from very global criteria of spatial orientation.

Our experiments were exploratory. Further research should inform us about the precise nature of the perception of stochastically specified events. A primary question is whether the poor performance of the human perceiver is typical for temporally presented patterns, or whether this result also appears with patterns of simultaneous dots.

FIGURE 5



The patterns having high loadings on the gestalt factor form one confusion group, those with low ones make up the two other groups. This difference is significant for each of the three noise levels taken separately. Pattern 6 is a case by itself.

Further, many kinds of successive presentations are possible. Some patterns from our study (e.g. square, circle, star, helix) are completely specified by the location only of the dots of which they are composed. One might present these dots in any order of succession without changing the pattern. A particular order here is comparable to a manner of *scanning* of the pattern. In other patterns, however (like dance, glue, bouncer), the nature of the pattern arises from the specific *order* of the positions, every different order giving a new kind of pattern. In our study no attempt was made to control this kind of variation.

While it is well known that the nature of the scanning process affects the spatial impression of a group of dots we do not know of any systematic research on this phenomenon. But there are some suggestive results from studies by Michotte and De Clerk (1951) and by Christian and Von Weizsäcker (1943). Michotte showed that a radar-sweep-like presentation of spatial patterns is detrimental to pattern identification, any pattern tending to be perceived as a circle. The second study is alleged to show that the position of a circular pattern relative to the centre of the scanning rotation affects the nature of the visual perception of the pattern. The conclusion of this study was somewhat unsatisfactory; one feels in spite of the depth of the theory brought forward by Von Weizsäcker, that we might learn something more precise about this kind of effect. The nature of the perception reveals an unconscious mathematician in our mind, Von Weizsäcker says—our conclusion is that this mathematician is not very good at statistics.

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SPECIAL ARTICLE

DIFFERENCES AND INTERACTIONS BETWEEN STRESSES*

BY

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This paper reviews a number of experiments in which different stresses have been applied to comparable subjects performing similar tasks. It is argued that, since the effects of the stresses are different, it is not legitimate to think of a single mechanism mediating reaction to stress. Experiments on the simultaneous application of two stresses show that the effects of heat appear to be independent of those of noise and sleeplessness, while the latter two conditions partially cancel each other. It is therefore argued that noise and sleeplessness affect the same mechanism in opposite directions, while heat affects some other mechanism. Tentatively, noise is regarded as over-arousing and lack of sleep as under-arousing.

INTRODUCTION

During the past twenty years or so, it has been demonstrated that each of a number of unpleasant environments affects human behaviour. High temperatures (Mackworth, 1950; Pepler, 1958), noise (Jerison, 1957; Grimaldi, 1958) and deprivation of sleep (Wilkinson, 1960; Williams, Lubin and Goodnow, 1959) are typical of the variables that have been studied and proved harmful. It has been usual, however, to consider each variable alone, and very often the investigation has enquired only whether the effects were undesirable, without analysing the type of effect more closely. Nevertheless it is desirable that one should understand the mechanism by which the effects of stress are produced. If that mechanism is the same for different stresses, the disorders of performance should be the same in the different cases. If they are different, the nature of the differences may shed some light upon the various mechanisms in play.

There is also a considerable practical problem of combined stresses. An environment which is hot may also be noisy: although there appears to be a critical temperature in the region of 81° F. above which performance deteriorates, ought this to be set at a lower level when noise is also present? Are the effects of different stresses multiplicative, additive, or do they perhaps even cancel each other?

Systematic research on this point has been rare, presumably because there are even more combinations of stresses than there are simple stresses, and therefore the time required for investigation becomes considerable. Even if single stresses are being compared, the same task and the same type of subjects must be used in each case, and thus the whole research must normally take place in one laboratory. The following results are therefore a summary only of Cambridge research, and are correspondingly incomplete. It seems reasonable to conclude already, however, that different stresses do affect different mechanisms, and even to suggest the nature of some of the mechanisms.

* This article, based upon a paper delivered to the Experimental Psychology Society at a meeting held in London on 4th January, 1962, was prepared at the request of the Editor. It is hoped to publish further Special Articles from time to time to draw together recent experimental work in closely related fields.

COMPARISONS OF STRESSES

There are certain tasks which have been studied in several different stresses and on similar populations of Naval Ratings. The first of these is a serial-choice task, which lasts half an hour. The subject sits before a panel, on which there are five brass contacts arranged in an equilateral pentagon. There are also five lights, one corresponding to each of the contacts. (In some cases the lights are next to the contacts, and in some cases mounted on a vertical panel in front of the subject. Although this later condition may perhaps require more practice, the particular results to be mentioned are not affected by it.) At the start of the period, one of the lights is on and the others are off. The subject touches the appropriate contact with a stylus, and the light goes out. Another light immediately comes on, receives its response, and so on. Thus the rate of work depends on the man himself, and provides a score. His errors also can be counted.

On this task the average rate of work remains constant during a period of half an hour under normal conditions (Broadbent, 1953; Pepler, 1959; Wilkinson, 1959). It is also the same in noise as in quiet (Broadbent, 1953, 1957) and in heat as at normal temperature (Pepler, 1959). But it is slower after loss of sleep (Pepler, 1959; Wilkinson, 1959).^{*} Furthermore, the size of the effect of loss of sleep increases as the work-period goes on and may be entirely absent in the first five minutes.

The incorrect responses, on the other hand, become more frequent towards the end of a work-period (Broadbent, 1953; Pepler, 1959). They are increased in noise, but this effect is greater at the end of the period and may be reversed in the first five minutes (Broadbent, 1953, 1957). They are not increased by loss of sleep (Pepler, 1959; Wilkinson, 1959), but do become more frequent in heat (Pepler, 1959). The latter effect remains constant in size throughout the work-period.

Thus each stress has its own pattern of behaviour: loss of sleep affects speed but not errors and only does so at the end of the task, noise affects errors but not speed and does so at the end, while heat affects errors but not speed and does so at the beginning. It must immediately be said that these results do not imply that scores on other tasks, which may also be called "speed" and "errors," will be similarly affected. But since the task has been held constant, it is at least clear that the stresses differ in their effects.

A second task which has been used in several experiments is a pursuit-meter, in which the subjects followed a target pointer with another pointer controlled through a lever. The target oscillated with a mean frequency of 0.5 c.p.s., and the whole display was viewed through a diffuser which made it very difficult to see. On this task the discrepancy between the two pointers was integrated over time and provided a score which may be called "error," although it is, of course, quite different from error in the serial-choice task: in the latter the subject had to do something to make an error, whereas in the pursuit-meter he would score highly in error if he did nothing at all. A second score was provided by the number of times the controlled pointer changes its direction of movement; this provides some indication of the extent to which the subject was in fact attempting to follow the target, and is called the "movement" score.

On this task the error score increases in heat (Pepler, 1959, 1960), after a loss of sleep (Pepler, 1959), in glare (Pepler, 1960) and under the distracting condition of hearing a faintly-spoken passage from a thriller (Pepler, 1960). Thus this score does

^{*}By "heat" in this context is meant an environment of 105° F. dry-bulb and 95° F. wet-bulb temperature. By "noise" is meant continuous wide-band noise of 100 db. re. 0.0002 dynes/sq. cm.; and by "loss of sleep" one complete night's deprivation.

not appear to differentiate between the stresses, except perhaps in that the effect of loss of sleep shows more tendency to increase during the run than does the effect of heat. (This, it will be remembered, was also true on the serial-reaction task.) Heat and sleeplessness differ most markedly, however, in the movement score; whereas in heat there is an increase in that score, loss of sleep produces no such increase (Pepler, 1959). Glare and distracting speech give a definite decrease (Pepler, 1960). Thus in this task also, different stresses give different patterns of behaviour.

The interpretation of these patterns is, however, not completely clear. In the case of glare and distraction, it seems likely that there is an interference with perceptual processes, and therefore the subjects not only do badly but also fail to perceive the extent of their error and do not try to correct it. By analogy one might suppose that the effect of sleeplessness is also perceptual, but that the effect of heat is not. Therefore the latter stress is the only one in which increased error is accompanied by increased attempts to correct that error. Unfortunately, the pursuit-meter task has not been used in noise of intensity and duration similar to that employed as a stress on the serial-reaction task. Thus it remains unknown whether noise would resemble heat or loss of sleep on the movement score of the pursuit-meter.

Despite these uncertainties, it is clear that different stresses have quite different effects; and in general that loss of sleep gives an inert type of behaviour while high temperatures and noise give active but inaccurate types.

INTERACTIONS OF STRESSES

These differences in type of breakdown must imply that the stresses either affect different mechanisms or else that they affect the same mechanism to different extents: say in opposite directions. If two stresses act by quite different mechanisms, their effects should be independent of each other; and if they operate by manipulating the same mechanism in two opposite directions (say, one by raising and one by lowering a general level of arousal), then their effects should tend to cancel out. Thus by applying the two stresses together we should be able to separate these possibilities. In fact as will be seen, the present evidence suggests that heat affects an independent mechanism of its own whereas loss of sleep and noise affect the same mechanism in opposite directions. It might be for instance that one of them reduces and the other raises the general level of arousal.

To consider first the evidence for the independence of heat and loss of sleep, Pepler (1959) applied both stresses together and was unable to show any statistically significant interaction of their effects on either the serial-choice task or the pursuit-meter. Furthermore, there is some evidence that the two stresses are differently affected by incentives. In all the experiments using the serial reaction task which have been described so far, the man performing the task had no information about his own success beyond that which he obtained from realization of his own errors and rate of work. It is possible to raise the subject's level of motivation by telling him every five minutes the number of correct and incorrect responses which he has made. When this stimulating and informative treatment is applied the man performs almost equally well whether sleepless or not (Wilkinson, 1961*a*). It is noteworthy that other forms of incentive, which do not inform the subject about his own performance, reduce the effect of loss of sleep (Corcoran, 1963*b*). Thus it is truly the motivating effects of knowledge of results, rather than the informative ones which oppose those of loss of sleep.

If now the effect of heat was mediated by the same mechanism as that of loss of sleep, it too should be reduced by incentives. Most unfortunately this experiment has not been carried out using the serial reaction task but it has several times with

other tasks (Mackworth, 1950; Pepler, 1958) and in no case has there been any sign of a reduction in the effect of heat by incentives. They improve performance; but they do so by the same amount whether the temperature is high or normal. Thus here again there is no evidence that the effects of heat have anything in common with those of loss of sleep.

When we compare the interaction of effects of noise with those of loss of sleep, however, the picture is quite different. Experiments on the application of both stresses together (Corcoran, 1962, Wilkinson, 1963) show that the effect of loss of sleep is less when loud noise is present; and a study of the effects of knowledge of results upon performance of the serial reaction task in noise (Wilkinson, 1963) shows that the effect of noise is greater when incentives are applied than when they are not. Thus the effects of noise and of loss of sleep oppose each other, at least in part.

Some authors, such as Hebb (1955), have suggested that men may be inefficient not only because of too low a level of arousal, but also because of too high a level (see Broen and Storms (1961) for a recent review and discussion of this possibility). Since sleep is certainly connected in some way with the general level of arousal of the nervous system, it is possible that one of the stresses raises the level too high whereas the other reduces it too low. While a case might in the abstract be made for regarding continuous noise as de-arousing (because it masks out the usual varied background of auditory stimulation), and loss of sleep as over-arousing (because it means more exposure to stimulation), this view encounters various difficulties. First, the effects of incentives already mentioned would be surprising on such a theory. Incentives surely ought to be arousing, and therefore the stress which they cancel out should be de-arousing; and that is loss of sleep. Secondly, for similar reasons we would expect the effect of an over-arousing stress to be greater when the number of stimuli per minute increases in a task which otherwise remains the same. But Corcoran (1963a) has shown that loss of sleep affects a task less when the rate of stimulation is greater. Thirdly, Wilkinson (1961b) has found that individuals with relatively high muscle tension are those who show less effect of loss of sleep; and this would be surprising if sleeplessness were de-arousing. Fourthly, in unstimulating tasks it has been found by Corcoran (1963b) that physiological measures show signs of lowered arousal after loss of sleep, even though in stimulating tasks the reverse may be the case (Malmö, 1959).

It is therefore a more plausible hypothesis that loud noise is over-arousing because it represents too high a level of stimulation; and that this effect is greater when incentives are applied since they may be supposed to bring the level of arousal up to its optimum even in the absence of noise. Loss of sleep on the other hand reduces the level of arousal to a dangerous extent, but is less likely to do so when incentives are present. Such a view is also consistent with the type of breakdown which occurs on the serial reaction task under the two stresses; with loss of sleep the man does badly by doing little whereas in noise he does badly by doing the wrong thing.

SOME DIFFICULTIES AND OMISSIONS

The most major difficulty in the foregoing account is that both sleeplessness and noise have their greatest impact at the end of a work-period than the beginning. If noise were hyper-arousing, it surely ought to have its main effect at the start of work before the task becomes familiar and monotonous. Some addition is therefore needed to the simple theory that sleeplessness and noise are opposite in their effects: there might perhaps be some other consequence of continuous work which makes the man inefficient and then sleeplessness or noise determines the way in which the inefficiency shows itself.

Another point which requires more examination is the relationship of heat to the other stresses. From everyday experience one would have thought that very high temperatures made it difficult to sleep, but moderately high ones were distinctly soporific: and it may therefore be only the particular condition of 105° dry-bulb and 95° wet-bulb that is independent of the arousal mechanism. Furthermore, although the experiment of Pepler (1959) shows quite clearly that, for instance speed on the serial-reaction task shows no interaction between heat and loss of sleep, yet there was an interaction (even though statistically insignificant) on the movement score of the pursuit-meter. Thus there may possibly be some marginal effects of heat which do interact with those of loss of sleep even though we can be sure of a large area of independence. It would also be desirable to study the combined effects of heat and noise, which ought to show no interaction if our other conclusions are sound. Indeed, Viteles and Smith (1946) did study various combinations of noise and temperature and concluded that there was no interaction between them. Their noise levels were all lower than those in the other experiments cited, and their tasks and subjects were, of course, different but at least it can be said that their conclusions raise no difficulty for our view that noise and sleeplessness are polar opposites while heat is a stress of a different kind.

It would also be desirable, as has been noted, to study the interaction of knowledge of results and heat on the serial reaction task, since so far this combination has only been used with other tasks. The interaction of incentives with noise should also be examined using incentives other than knowledge of results: although as already indicated other incentives have been shown by Corcoran to reduce the effect of loss of sleep, it is likely that different types of incentives are different in their nature. Williams, Lubin and Goodnow (1959) did not find an interaction of loss of sleep and knowledge of results, perhaps, as they say, because their knowledge of results did not possess a very strong incentive character: or possibly, of course, because of differences in task and subject sample from those used in Cambridge.

Other studies which should be done in noise concern the individual differences, and the effect of varying the frequency of signals in the task. If the general view of sleeplessness as under-arousing and noise as over-arousing is not an over-simplification, those men with the highest muscle tension in noise should be the most impaired in efficiency; since in sleeplessness the reverse is true. (This point was drawn to my attention by Air Cdre. W. K. Stewart, R.A.F.) Furthermore we would expect a task with a high signal frequency to show bigger effects than a task with a low frequency. To some extent the latter comparison has been made and the prediction confirmed by Jerison (1957, 1959). There were, however, other differences between his tasks, and he himself considered that the necessity for division of attention was the crucial variable in making a task vulnerable to noise. Further work on this point is in progress.

Another profitable field for further study would be the use of drugs to raise and lower arousal, in combination with other stresses. To put the point crudely, it might be that sedatives will reduce the ill effects of noise on efficiency. It is noteworthy that a study of the effects of a tranquillizer on the serial-reaction test (Steinberg, 1959) has shown that the ill effects are much less when knowledge of results is given, so that the action of the drug appears similar in this respect to that of sleeplessness. Admittedly the effects in this case were on errors rather than speed, and this points to a difference between the two variables. On the other hand, the sample of subjects was very different from those used in the other experiments cited, and this might be the reason for the difference.

All these areas of research clearly require investigation, and any of them may yield

results inconsistent with the simple theory based on the concept of arousal, which has been put forward in this paper. Perhaps the main weakness in such a theory is already evident, however: it can explain both rises and falls in efficiency with equal ease when almost any treatment is applied to a man. This point has already been discussed (Broadbent, 1963); it obviously means that very stringent precautions must be taken to tie down the exact level of arousal at which particular predictions are supposed to apply. So far, little has been done in this way. Whatever the fate of the concept of arousal, however, it does seem clear that heat, noise and sleeplessness are not to be lumped indistinguishably together under the single label of stress; for each produces distinctive effects. Heat on the whole seems to impair performance by a mechanism of its own, while noise and sleeplessness act on some common mechanism in opposite directions.

TABLE I
THE SUMMARIZED EFFECTS OF THREE STRESSES ON THE SERIAL-REACTION TEST AS FAR AS PRESENT EVIDENCE GOES

	Speed	Errors	Place in work period where effect appears	Effect of incentives	Interaction with	
					Heat	Noise
Heat	No effect	Increased	Throughout	(No change in effect*)	—	(None)
Noise	No effect*	Increased*	End*	Increases effect	(None)	—
Loss of sleep	Reduction*	No effect*	End*	Decreases effect*	None	Reduced effect*

Results in brackets are not from the serial reaction test and are therefore not strictly comparable.

* These results are from more than one experiment and are therefore particularly trustworthy.

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SHORTER ARTICLES AND NOTES

DIRECT TRANSFER FROM A HORIZONTAL-VERTICAL DISCRIMINATION TO A BRIGHTNESS DISCRIMINATION IN THE RAT

BY

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Eight rats were trained on a discrimination between horizontal and vertical striations, and subsequently between black and white vertical striations. Subjects originally trained with horizontal positive preferred black to white and subjects trained with vertical positive preferred white to black.

The present note reports briefly on a transfer effect in rats, observed incidentally in an experiment designed for a different purpose. The rats were trained in a jumping stand to discriminate between horizontal and vertical striations, and when training was complete, were tested on a brightness discrimination.

METHOD

The subjects were eight experimentally naive male hooded rats from the colony maintained at the Institute of Experimental Psychology, Oxford. They were 3 months old. After pretraining to jump to open doors, they were trained on the horizontal-vertical discrimination: 10 trials a day were given by a modified correction procedure for 12 days, by which time all subjects had reached a criterion of 18 correct responses out of 20 trials with the last 10 all correct. (For further details of apparatus and procedure see Mackintosh, 1964.)

The stimuli were cut from black or white $\frac{1}{4}$ -in. perspex sheet and mounted on brown hardboard doors 14 cm. square. Four stimuli were used: a black and a white pair of horizontal and of vertical bars. Each stimulus was made up of 5 separate 10×1 cm. rectangles (set either vertically or horizontally) mounted with a gap of 1.25 cm. between

TABLE I

BRIGHTNESS DISCRIMINATION
Mean trials to criterion

Group	H—B	H—W	V—B	V—W
Trials to criterion ..	45.0	79.0	59.0	43.5

rectangles: thus the outline of each stimulus formed a 10×10 cm. square. Four subjects were trained with horizontal positive: four with vertical positive: all subjects were trained with both black and white stimuli; on any one trial both stimuli were of the same colour (both black or both white), which brightness was used being determined by a random order.

After the 120 initial trials, all subjects were trained (to the same criterion) by a non-correction procedure to discriminate between the black and the white vertical bars. Half of each original group was trained with black positive, half with white positive. There thus four subgroups, each with two subjects:—H—B; H—W; V—B; V—W.

The results for this stage of the experiment are shown in Table I. An analysis of variance performed on these scores showed that neither of the main effects were significant (for positive stimulus during training, $F = 1.1$; for positive stimulus during testing, $F < 1$); but the interaction between the two was significant at the 5 per cent. level

($F = 8.1$, $d.f. 1,4$). Subjects trained with horizontal positive preferred black to white; subjects trained with vertical positive preferred white to black.

DISCUSSION

Such a small number of subjects are involved (two per subgroup) that these results might reasonably be ascribed to chance were they not in line with an earlier finding (Law, 1954). Law found that rats trained initially to discriminate between black and white cards, preferred horizontal striations when they had been trained with black positive, and vertical striations when they had been trained with white positive. The present results were obtained after training initially on the horizontal-vertical discrimination, and then testing on black vs. white. A further difference between Law's experiment and that reported here, is that in the former, the horizontal and vertical striations were made of alternating strips of black and white paper. Under these circumstances it is conceivable that the rat fixating or scanning a small segment should receive brighter stimulation from one card than the other; however, this possible explanation would seem to be ruled out by the present results which were obtained using both black and white bars on a neutral background. If, for example, the rat scans the bottom of the card, in Law's situation it might receive predominantly black stimulation from the horizontal striations, but alternating black and white patches from the vertical. With the stimuli used here, this would hold when the black bars were used, but precisely the opposite effect should occur when white bars were used. Thus no brightness differences arising from these types of cue could have been utilized in the solution of the original problem.

In an earlier experiment (Mackintosh, 1962), the author trained rats first to discriminate between black and white cards, and subsequently between three grey horizontal or vertical rectangles. No suggestion of direct transfer was found here; nor (unpublished data) does this transfer effect appear when a single horizontal or vertical rectangle is used, whether the rectangles are both grey or either black or white. It seems likely, therefore, that the transfer found in the present experiment only occurs with horizontal and vertical striations, and perhaps only when the striations are black or white (and not grey).

No attempt is here made to offer an explanation for these findings, although they must eventually be included in any specification of how rats analyse differences within various stimulus dimensions. Their immediate importance lies in showing that considerable caution is needed in interpreting results of discrimination experiments of this type. For instance, Dodwell (1961) has claimed that the rats' supposed preference for horizontal striations in the jumping stand may be due to their having been pretrained to jump to open doors, and claims that in fact they prefer vertical striations. But Dodwell pretrained his rats to jump to a diffuse pattern of white dots as opposed to a black door, i.e. to the brighter of two stimuli: thus the fact that they learnt the horizontal-vertical discrimination faster with vertical than with horizontal positive does not prove a natural preference for vertical, but (as here) some equivalence between vertical striations and brightness.

It is interesting that precisely the opposite effect occurs in octopuses (see Sutherland *et al.*, 1963): for these animals there seems to be a partial equivalence between horizontal and white on the one hand, and vertical and black on the other.

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VERBAL VERSUS MANUAL REACTION TIME IN SCHIZOPHRENIC AND NORMAL SUBJECTS

BY

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On the assumption that schizophrenia involves a disturbance in interpersonal relations it is predicted that the verbal reaction time responses of schizophrenic subjects should have disproportionately longer latencies than manual reaction time responses as compared to normal subjects. Using data presented in this *Journal* by Venables and O'Connor (1959), the prediction is confirmed for patients whose reaction time level is relatively fast, but not in those with relatively slow reaction times. Several possible mechanisms are postulated to account for the results, including differential reinforcement, amount of practice, and complexity of verbal responses, and degree of pathology of the patients.

Schizophrenia may from one point of view be considered as a deficit in the ability to carry on interpersonal relations. From such a standpoint, one could predict that a kind of response which was basically involved in interpersonal transactions would be more adversely affected in schizophrenics than a response which was relatively little involved. Since speech is the principal mode of establishing and maintaining interpersonal relationships, deficits, for instance, in the verbal reaction time of schizophrenic subjects might be expected to be greater, relative to their manual reaction time, than any such deficit in normal subjects.

Such an experiment has been on our agenda of planned studies for some time. In the meantime, however, a study having a bearing on the problem but with a different theoretical orientation was published in this *Journal* (Venables and O'Connor, 1959). The authors were concerned primarily with reaction times to auditory and visual stimuli by schizophrenic and normal subjects. Two kinds of reactions were tested, a finger press and a speech response (saying the nonsense syllable "bip"). Each subject was given ten RT trials under four conditions: Auditory Manual (AM), Visual Manual (VM), Auditory Speech (AS), and Visual Speech (VS). Since mean RT for each condition is presented for each of the 48 schizophrenic and 20 normal subjects, this permitted us partially to test our own hypothesis by analysing their data.

This hypothesis as applied to the present situation requires the assumption that the relatively meaningless nonsense syllable "bip" is to some degree affected in the same way as are more meaningful and communicative speech responses by the disturbances in interpersonal relations characteristic in schizophrenia. Our hypothesis requires that when we subtract manual from speech RTs we should find higher differences for the schizophrenic than for the normal subjects. Initial examination revealed that the mean of AS minus AM scores was 0.110 and 0.100 sec. for the schizophrenic and normal groups respectively. Similarly, the mean of VS minus VM scores was 0.083 and 0.090 sec. respectively for the two groups.

We were puzzled by this apparent failure to find confirmation of our hypothesis. However, we were impressed by the wide range of mean RTs among the schizophrenic subjects used by Venables and O'Connor. In our own studies, we have gained the impression that among patients in whom RTs are unduly long, extraneous variables are often interfering with performance, to the extent that in extreme cases the meaning of "reaction time" is probably considerably altered. We have repeatedly found that the data from patients with faster RTs under favourable conditions are generally more consistent than are data from patients with quite long RTs.

We decided therefore to test our hypothesis using only those subjects who met a more rigorous criterion of performance. Since Venables and O'Connor had used a median division point between 0.39 and 0.40 sec. on the AM task to test auditory *versus* visual RT performance, we decided to use the same point. Mean RT for all four conditions for these groups and for normals are shown in Table I. For the subjects with an AM RT of 0.39 sec. or less, we found that the difference between the schizophrenic and control groups with

respect to speech-minus-manual RT is significant at the 0.01 level ($t = 3.77$, $d.f. = 22$). (Because of significant heterogeneity of variance the $d.f.$ for all t -tests were calculated according to the formula given by Dixon and Massey (1951, p. 105.)) Similarly, for the visual condition, the difference is also significant at the 0.01 level ($t = 2.93$, $d.f. = 19$). Thus, the hypothesis is supported for both modalities.

Since the speech-minus-manual differences are in the same direction for both schizophrenic and normal groups it might be thought that the difference is merely a function of RT level rather than diagnosis. If this were true one would expect positive correlations between the RT levels and speech-minus-manual scores for each group. Product-moment r 's were computed between manual RT level and the speech-minus-manual score for each group under each stimulus condition. For the normal subjects the r 's were -0.10 and

TABLE I

MEAN REACTION TIME (IN SECONDS) FOR NORMAL AND FOR TWO GROUPS OF SCHIZOPHRENIC SUBJECTS UNDER THE FOUR CONDITIONS

Condition	Normal	Schizophrenic	Schizophrenic
	AM < 0.40	AM < 0.40	AM ≥ 0.40
AM ..	0.245	0.310	0.717
AS ..	0.334	0.505	0.796
Diff ..	0.089	0.195	0.079
SD Diff ..	0.051	0.101	0.215
VM ..	0.306	0.380	0.660
VS ..	0.401	0.546	0.719
Diff ..	0.095	0.166	0.059
SD Diff ..	0.036	0.091	0.285

-0.17 for the auditory and visual stimuli respectively; for the schizophrenics the corresponding r 's were 0.12 and -0.49 . The latter figure is significantly different from zero at the 0.05 level. Thus if there is any relationship at all between RT level and the difference score it would seem to be negative. At any rate it does not seem to be the case that higher RT levels are associated with higher difference scores within each group.

What about those patients whose AM RT was 0.40 sec. or more? Neither of their speech-minus-manual differences is different from the corresponding values for the normals, but both are different from the 0.39-or-less group at the 0.05 level in the first instance and between the 0.10 and 0.05 levels in the second. Our hypothesis is that extraneous factors might have been interfering with RT and that these subjects presumably did not constitute a homogeneous group which would be comparable to the patients with better RTs. This is supported by the large differences in the variances of the two groups. For the auditory condition, $F = 4.53$ ($p < 0.01$) and for the visual condition, $F = 9.81$ ($p < 0.01$).

The reliability of the RTs could be indicated in the following way as well. If effects on RT were consistent for the auditory, visual, manual and verbal conditions, then the 0.39-or-less group and the 0.40-or-more group should both reflect this consistency of effect. In terms of our own interest, this means that the AS-minus-AM scores should be correlated with the VS-minus-VM scores to the same degree in both groups. Under ordinary conditions the correlation should be higher for the 0.40-or-more group since its range is so much greater. Actually, the correlation of these scores was 0.54 for the 0.39-or-less group ($p < 0.05$) and 0.26 for the 0.40-or-more group. The latter correlation is not statistically significant. Thus it appears that the 0.40-or-more group is producing less reliable data

than the schizophrenics with faster RTs. However, whether this unreliability has completely obscured a real difference from the normal controls or whether in fact no real difference exists is still an open question.

There would seem to be several possible hypotheses to account for the disproportionate deficit on verbal RT shown by the faster schizophrenic group. One of these might suggest that because of a long history of negative reinforcement of verbal responses due to repeated painful experiences in interpersonal relations, verbal responses have become disproportionately inhibited and have acquired longer latencies in schizophrenic subjects.

A second hypothesis is that frequency of association between classes of stimuli and responses is a factor in the disproportionate verbal slowing in the RT of schizophrenics. Such a view might hold that speed of verbal response to various stimuli is not demanded or practised in daily life whereas speed of manual response is, and that the RT experiment is reflecting this previous learning. This could then account for the increased slowing of verbal RT in schizophrenics if we assume, not unreasonably, that verbal responses have occurred proportionately less frequently in schizophrenics perhaps for the reasons outlined above. According to such a view, practising RTs involving a response such as "bip" should reduce normal subjects' RTs for this response to a level closer to manual RTs and perhaps reduce other verbal RTs as well through response generalization.

A third hypothesis would not postulate any special properties with respect to verbal responses except to assume they are more complex than manual responses. It has been found, both in the present study and by Wells (1924), that verbal RT is longer than manual RT. Although this does not prove that verbal RT is more complex, since different methods of measurement are involved in the two modalities, it suggests such a possibility, as pointed out by Woodworth (1938). That the verbal response "bip" is more complex seems reasonable on *a priori* grounds since it involves the coordination of more diverse muscle groups than a finger-press response. Thus it may be hypothesized that schizophrenics give disproportionately slower RT the more complex the response required.

The foregoing hypotheses have not taken the results of the 0.40-or-more group into account. Although their possible unreliability makes it difficult to form any firm conclusions, the fact remains that this group does not show the same exaggerated slowing of verbal response *relative* to manual response that is demonstrated in the 0.39-or-less group. It will be noted from Table I that these subjects are considerably slower in *both* kinds of responses than are the other groups.

This may possibly be due to the pervasive effect on psychological functions which marked psychotic disturbance carries with it, as contrasted with the possibility for differential effect on functions having different degrees of complexity or involvement of higher cortical processes, in the case of milder forms of disturbance. Another possibility is that the deficit in verbal responses, found in both schizophrenic groups, is due to the factors peculiar to verbal responses hypothesized above while manual responses suffer a gross deficit from some different source which is present mainly in patients with more severe pathology.

We have variously postulated differential reinforcement, amount of practice, complexity of response, and degree of pathology as possible variables accounting for the observed relationships occurring in the present study. To clarify the nature of these relationships, further research is needed in which these variables are systematically varied or controlled.

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THE TACTILE PERCEPTION OF SIZE: SOME RELATIONSHIPS WITH DISTANCE AND DIRECTION

BY

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The results of a series of experiments carried out by Bartley (1953) demonstrated a tendency for the size of a tactually perceived object to be underestimated, with increased distance of the object from the eyes. He took this to indicate that visual imagery played a part in the spatial organization of tactile data. In the present investigation this effect of distance was further examined under various conditions with sighted and blind subjects. The tendency was found to exist, but there is clear evidence that it is not due to the participation of visual imagery. No hypothesis was found to explain the tendency adequately. An interesting difference emerges between the situation where the subject extends his arm when making the comparison and that in which the arm is retracted when making the comparison.

INTRODUCTION

Bartley (1953) reported a number of experiments in which blindfold subjects compared the sizes of square wooden blocks at different distances from the eyes by running the index finger round the edges of the blocks. The experiments varied somewhat in detailed design, but the results, together with those from other experiments on the same theme, led Bartley to the conclusion that the tactile estimation of the size of objects is influenced by the distance of these objects from the eyes: the further away the object is, the more its size tends to be underestimated. The length of the arm is, of course, a limiting factor. This tendency showed in all the experiments, to a significant degree in some. Bartley took this to be evidence that vision was influencing the spatial organization of tactile perception. The experiment reported here is an attempt to explore this phenomenon further. Two approaches were used: (a) subjects made tactile size judgements with the arm flexed and extended at various angles to the body; (b) the experiments were repeated with blind subjects.

SUBJECTS

In the experiments to be described, the 38 sighted subjects were almost entirely undergraduate or postgraduate students; ages were in the 20's and 30's, with a concentration towards the lower end of the range. There were 21 men and 17 women. (Twenty-two subjects were used in only one condition. The remaining 16 were each used in two conditions, with an interval which was never less than 30 minutes. Subjects being used for a second time produced a sample of scores which clearly came from the same population as the scores from subjects tested once only.)

In one part of the procedure the subjects were 11 pupils (8 boys, 3 girls) at a school for the blind. Seven were congenitally blind, one had been blind from four months after birth, one from age two years, and one from age three years, all well within the limits suggested by Schlaegel (1953) who thinks that visual imagery is not available to those becoming blind before the age of about six years. The ages of these subjects ranged from 10 to 18, with a cluster around 13.

APPARATUS

The stimuli used through these experiments were strips of Duralumin 1.5 in. wide, with a flange at right-angles 0.75 in. wide. These sections were cut to lengths between 1.5 in. and 1.9 in., varying by steps of 0.1 in., so that there were five lengths, each machined to an accuracy of 0.002 in. Each length was then fastened, through holes drilled in the 0.75 in. flange, to a wooden base. The upper edge of the stimulus thus stood 1.5 in. proud of the base, clues from which could be ruled out. (The Standard was 1.7 in. long, the Variables 1.6 in. and 1.8 in. The 1.5 in. and 1.9 in. lengths were introduced randomly to prevent

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the subject becoming aware of the limited number of variables being used. Judgements involving these were not taken into account in the recorded results.)

PROCEDURE

In preliminary experiments it was found that the tendency to underestimate the size of the more distant stimulus objects occurred even when the subject simply rested his palm on the objects, rather than running his finger round them as in Bartley's experiments. This was therefore used rather than Bartley's less controlled method. Each subject was blindfolded and seated. His flat palm was placed on the Standard stimulus for two seconds; it was then moved during a five-second interval and placed on the Variable for a further two seconds. The subject judged if the Variable was longer, equal to or shorter than the Standard. Under each condition 32 trials were given to each subject (not counting the unrecorded judgements made with extreme lengths). After every four trials the standard was moved from the near to far position, or *vice versa*. For each position of the Standard, the two Variables were presented randomly, eight times each.

CONDITIONS

Under each condition tactile size judgements were compared with stimulus objects at two different positions as follows:—

- A. 2.5 in. and 20 in. ahead, both at eye level.
- B. 2.5 in. and 20 in. ahead, both at table level.
- C. 2.5 in. ahead and 20 in. to the right, both at table level.
- D. 2.5 in. ahead at table level, and, at arms-length, with the arm held straight down by the subject's side.
- E. 2.5 in. and 10 in. straight ahead.
- F. Condition C with blind subjects.

Condition A resembled Bartley's method. Conditions B, C, D and E explored the possibility that size judgements depended on nearness and orientation of the objects to the body. Condition F attempted to test Bartley's hypothesis directly.

RESULTS

The figures indicate the number of subjects showing *predominantly* underestimation, no effect, and overestimation of size when the arm is extended as opposed to flexed.

Condition	Under estimation	No effect	Over- estimation
A. Eye level (N = 12)	7	2	3
B. Table level 20 in. (N = 12) ..	8	2	2
C. To right (N = 12) .. .	9	2	1
D. Downwards (N = 6)	5	0	1
E. Table level 10 in. (N = 12) ..	7	2	3
F. Blind. To right (N = 11) ..	9	0	2

(There were no significant differences between male and female subjects.)

If visual imagery is having any effect at all, it must be very small. The obvious hypothesis to explain the results is that most subjects tend to underestimate tactile size when the arm is extended as compared with when it is flexed. The importance of flexion and extension can be shown in another way. The Standard was always presented first, but on half the occasions with flexed arm and half with extended arm. In the "Standard near" position, an arm extension is made just before the Variable is judged; in the "Standard far" position, a flexion precedes this judgement. In the following table, the total

numbers of under- and over-estimates are given for "Standard near" and "Standard far" separately for each condition.

	Conditions					
	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>	<i>E</i>	<i>F</i>
	U O (384)	U O (384)	U O (384)	U O (192)	U O (384)	U O (352)
Standard near	35 39	36 39	42 25	25 13	39 47	45 27
Standard far	50 22	56 25	50 24	31 17	62 50	42 34

U = underestimates.

O = overestimates of object when arm extended.

The total judgements under each condition is shown in brackets.

In conditions *C*, *D* and *F*, the tendency to judge as smaller when the arm is extended is independent of whether the arm is flexed or extended between judgements. But in conditions *A*, *B* and *E*, which are the straight-ahead conditions, the effect shows only if the arm is flexed between judgements; that is, the far object is judged as smaller only if it is felt first. (Control experiments with standard and variable at the same distance showed that time errors were negligible.)

CONCLUSIONS

If the arm is extended out to the side or held straight downwards while making a tactile size judgement, the size of an object is likely to be underestimated as compared with a judgement made with the arm flexed and the object at table level. When, however, the arm is extended straight ahead, either at table or eye level, the distant object is underestimated only when it is judged first. These results suggest that:

- (1) Vision does not play an important part in the effect. This is confirmed with the results from blind subjects (though it should be noted that they were younger than the sighted subjects).
- (2) The effect is more marked when the arm is extended sideways or downwards.
- (3) When judgements are made with objects straight ahead, the effect shows only if the arm is extended then flexed.

Special thanks are due to the staff and pupils of Linden Lodge School for their most helpful co-operation.

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BOOK REVIEWS

Conditioning and Learning. By E. R. Hilgard and D. G. Marquis. Revised by Gregory A. Kimble. London. Methuen. 1961. Pp. ix + 590. 60s.

Some of this book is *Conditioning and Learning* brought up to date and therefore valuable. This is particularly true of the chapters on conditioning experiments which occurred in the original edition; it is not true of the new chapters on reinforcement and motivation.

Hilgard and Marquis attempted to present the experiments which appeared important, without theoretical bias, either in selection or evaluation. This didn't make inspiring reading, but it was probably salutary at the time, and would be even more so in psychology today when "scientific explanations tend to come in clusters" seems the truest thing Hull ever said. There is a danger of ignoring facts for the sake of a more coherent theory, and a book like *Conditioning and Learning* should reduce the danger, by making facts less easy to ignore. Unfortunately, Kimble's revision has such a theoretical bias that it is likely to make things worse. The bias is apparent in his definition of learning. (Hilgard and Marquis didn't attempt a definition which makes one change for the worse.) It is "a relatively permanent change in behaviour potentiality which occurs as a result of reinforced practice." The use of the term "practice" makes it difficult to conceive of learning by exploration. It is strange (and ominous) that he fails to notice this, since he has just pointed out a bias in the opposite direction in the definition given by Maier and Schneirla.

It is not that he always fails to give the experiments refuting the Hullian view (though he does sometimes, as in his discussion of the continuity/non-continuity controversy) but rather that he neglects to draw the appropriate conclusions. For instance he gives evidence against the drive reduction theory of reinforcement, and concludes "The experiments described in the previous sections succeeded in demonstrating the partial validity of several hypotheses without seriously invalidating others." Such caution invites agreement and yet what he says is plainly false. The drive reduction theory is seriously invalidated, the stimulus theory is not.

The bias is revealed even more strikingly in his assumptions about drive stimuli. In a preliminary discussion he considers Hebb's notion of drive as "an energizer, but not a guide" and suggests that this is inadequate because "the hungry rat, given all three opportunities, eats rather than drinks or copulates, suggesting that motives somehow provide for the direction of activity. The mechanism commonly accepted as subserving this function of guiding motivated behaviour is that of drive stimulation (S_D). Is this really commonly accepted? It certainly might be if this book were thought to provide an adequate survey, since there is no mention in it of specific hungers, the behaviour of rats without stomachs and other relevant experiments. Neither the sufficiency nor the importance of drive stimuli should be assumed without evidence. But no evidence is given and even the "cognitive theorists" are said to believe that under high drive "the animal responds to its drive state (S_D) to the exclusion of everything else." Having made the assumption he considers whether drive stimuli act like other stimuli. For instance he tries to show that discrimination can be made between different intensities and quotes an experiment where a discrimination is made between 11.5 and 47.5 hr. of deprivation. A few pages previously he had accepted that stomach contractions do not increase above about 6 hr. of deprivation. So that the discrimination cannot be based upon intensity of drive stimuli, since he has already implied, without qualification, that stomach contractions are these stimuli.

After this kind of thing it is interesting to see what he says about the different theories. In the first edition Hilgard and Marquis felt that "expectancy may become a very important aid in the interpretation of experimental results in learning." But now "cognitive theory of the sort espoused by Tolman . . . is either sleeping or dying" (Kendler). Kimble explains that the development of the theory has been neglected because its exponents have been too busy trying to "discredit" the "S-R effect position" an attempt based on a misconception about the nature of scientific concepts, and having the effect of strengthening S-R theorizing "in spite of the fact that at least half of the studies have produced results which were negative to the S-R position. This is because the S-R theorists had to formulate more sophisticated theories" which minimized the differences so that now their concepts "have a distinctly cognitive ring." Thus Tolman was right and the S-R theorists have translated his theory into their own terms without acknowledgment. Needless to say, this is not Kimble's conclusion, but it seems to follow from what he says.

As one might expect Hull has 94 page references in the index which is twice as many as anyone else and nearly twice as many as he had in the first edition. Skinner has fewer than he had before and there are only two page references to Hebb. Also neurophysiology is relatively neglected, in spite of an implicit claim to the contrary.

This is not, therefore, a satisfactory book. It provides a useful account of many experiments but the thought is too dominated by that of Hull to provide the "critical exposition" that Hilgard and Marquis claimed to give.

A. W. STILL.

Determinants of Infant Behaviour. Edited by B. M. Foss. London. Methuen. 1961. Pp. xv + 308. 42s.

This book consists mainly of seven papers presented to a study group convened by the Tavistock Child Development Research Unit, supported by the Ciba Foundation and held in September, 1959. The meeting was held under the chairmanship of Dr. Bowlby and was designed to discuss the problem of mother-infant interaction from the viewpoints of those working on the behaviour of human infants or young mammals, and of clinicians concerned with the effects of early deprivation on the human child.

There are four sections, each including a few papers and a full discussion on each where this was available. In the first, on neonate behaviour, H. Blauvelt and J. McKenna discuss the capacity of the newborn human infant to orient towards its mother and make her aware of its capacities; M. Gunther relates success in breast feeding with breast structure, and H. F. Prechtl considers effects of prenatal and paranatal complications. In the second section, on animal behaviour, a paper by J. S. Rosenblatt, G. Turkewitz and T. C. Schnierla describes the effects of the nutritive mother- or brooder-kitten relationship on a kitten's later social life and H. F. Harlow discusses the effects of maternal deprivation on experimental neurosis in monkeys reared with cloth or wire mothers. The third section is on social behaviour, beginning with case notes on a deprived child's gradual, incomplete assimilation into an institution by G. Appell and M. David, and a paper by the same two authors on the effects of institutional care, with particular reference to a multiplicity of caretakers and amounts of social contact and isolation, a problem which is taken up again in the following paper by H. L. Rheingold in a comparison of social and exploratory behaviour in two groups of infants differing in the amounts of stimulation with which they were provided; the first being institution children, and the second "only" children living in their own homes. Finally in this section J. A. Ambrose considers the waning of the smiling response at different ages in institution and home-reared infants, and R. A. Hinde presents a short contribution on changes of responsiveness consequent upon performance, mainly in birds. The final section consists of a long theoretical paper by J. L. Gewirtz bringing the effects of normal stimulation, privation and deprivation on the acquisition of social motivation and attachment into the focus of an instrumental learning model with emphasis on reinforcement. Dr. Bowlby replies briefly to this contribution.

The book appears to be a straightforward account of the symposium with all the advantages and drawbacks of this; amongst the latter some unevenness of matter and presentation, all the difficulties involved in reporting discussion and the fact that some substantial information was given on film. On the credit side there is a stimulating freshness and variety of approach and subject-matter and some genuine difference of opinion.

In spite of a solidly based approach to the problem of mother-infant interaction where the most important factors discussed are gross bodily structure, sensory capacity and nervous mechanism there is at times a feeling that the book is transitional, perhaps premature. At times the outlook expressed seems at odds with the material presented, for example, the ethologically oriented view expressed that social attachments may arise from innate releasing mechanisms and also discussion in terms of learning theories and reinforcement seem, in this setting, curiously dated, or almost irrelevant. But this occasional feeling of discrepancy is exciting, and raises hopes for the next symposium.

M. A. VINCE.

Conceptions of Perceptual Defence. By William P. Brown. British Journal of Psychology Monograph Supplement No. 35. London. Cambridge University Press. 1961. Pp. viii + 106. 25s.

After an era of doubt and controversy during which the original proponents of perceptual defence have fought, suffered ridicule, wavered, and, in certain notable cases, gone over to the other side, it is encouraging to find a writer without any doubts whatever as to the reality of the phenomenon in question.

Brown describes his monograph as having two aims, first that of summarizing the experimental evidence with a view to establishing the form of the relationship between stimulus and response in perceptual defence experiments, secondly that of examining a number of the traditional explanations for "perceptual defence" phenomena.

As to his first aim he is to be congratulated on an excellent review of the more important work in this confused and difficult area, and a useful airing of the semantic confusion that has surrounded the terms perceptual defence, sensitization, and vigilance.

From a discussion of methods by which the recognition threshold may be measured, from which he omits any judgement as to their relative values on the grounds that they show no appreciable differences in reliability, he goes on to outline the evidence for the effects upon the recognition threshold of non-emotional factors.

The title of this particular chapter (no. 3) is perhaps a misnomer since it includes work showing the effect upon recognition of such clearly emotional influences as ego involvement, frustration, deprivation, reward, and punishment. Although Brown explicitly classifies the contents of this section as excluding the variable of stimulus emotionality it is felt that in view of his subsequent discussion of stimulus emotionality this classification of variables is somewhat arbitrary and misleading. While some classification of the essential variables is obviously desirable it may well have to wait upon the discovery of their precise role in the mechanism of threshold regulation.

It is when outlining the evidence for and against perceptual defence that he makes his most valuable contribution, an analysis of the intra-subject and intra-stimulus differences observed in the relationship between stimulus emotionality and threshold change. His conclusion is that there exists considerable evidence for the existence of an inverted "U" curvilinear relationship between these two variables. In this context he emphasizes the importance of independent measures of stimulus emotionality for, without such measures, as he puts it, positive results may prove the hypothesis of perceptual defence but negative ones can never disprove it.

After the facts come the explanations. Brown offers ten traditional theories. Of these, three reject and seven accept the hypothesis of perceptual defence. The latter, extensive in their various terminologies and viewpoints, have one thing in common, a failure to explain *all* the facts. At the end of this series of post-mortems he makes its *raison d'être* clear—a demonstration of the gulf which over the past 12 years, has grown between experimental progress and theory-making. The point is well made and no doubt deserved but some forewarning would have helped the reader towards a more appropriate orientation in the preceding chapters.

Brown's own predilection is towards a more adequate behaviour theory in which perceptual defence, as an epi-phenomenon, is brought into relation with the wider fields of memory, association, learning and projection. In this context he gives the impression that, while strongly advocating a more determined theoretical approach to the problems of perceptual defence, behavioural and physiological explanations are alternatives and the former preferable to the latter. In the reviewer's opinion this is a pity for there is perhaps no single area in psychology which offers a greater opportunity for a rapprochement between the data of experimental psychology, clinical practice, and neurophysiology than does this field of perceptual regulation.

With the ultimate goal perhaps of explaining those anomalies of perceptual regulation which characterize certain mental disorders, neurophysiological and psychological theorizing should be regarded as complements rather than alternatives, with the empirical data accruing from research in each discipline providing a stimulus for better experimentation and theorizing on both sides.

As a modest addendum to his monograph the author describes an experiment of his own which exemplifies all that he finds methodologically desirable for modern sophisticated research in this area. With the data from this study he then illustrates what can be done with a little imagination, a handful of assumptions, and the language of behaviour theory.

In all this is a stimulating monograph of considerable value to workers in this area.

N. F. DIXON.

Psychopathology and Psychiatry. By I. P. Pavlov. Translated by D. Myshne and S. Belsky. London. Central Books (Foreign Languages Publishing House). 1962. Pp. 543. 21s.

The posthumous publication of the works of a major figure in any discipline must be assessed from two points of view. Firstly their historical impact must be considered, and secondly they must be judged in terms of their contribution to the current state of the

subject. Much of what we know of Pavlovian theory is by means of his influence on contemporary workers. To read his own work, even in translation, demands that we examine it with the same critical eye that we would accord to any present-day writer.

From this point of view the main short-coming of these lectures is that they are based on inferences rather than on experiments. The opening paragraph of the book reads: "Regarding the language of facts as the most eloquent, I shall take the liberty of proceeding directly to the experimental material, which gives me the right to speak to you on the subject of my present communication." The facts, however, are concerned with experiments on conditioned reflexes in dogs, and not with psychopathology in man. Pavlov does not at any stage appear to have performed experiments on patients. On this basis he offers an interpretation of various psychopathological symptoms and mental diseases, especially schizophrenia. Anyone interested in an explanatory rather than a classificatory theoretical framework of mental illness will be impressed and stimulated by the book. At the same time the Pavlovian theory of psychopathology shares the fate of other all-embracing psychological systems: its explanatory hypotheses tend to be so general as to become practically meaningless.

For instance, Pavlov's opinion of the nature of schizophrenia is that in certain types of "weak nervous systems" it develops from the generation of an excessive amount of protective cortical inhibition. Protective inhibition has the effect of diminishing both the excitatory and internal inhibitory cortical processes. Thus the model accounts for slowness, catatonic stupor, and states of withdrawal. At the same time it can explain over-excitation and over-generalization through induction and weakened internal cortical inhibition. But it does not specify in what conditions one or the other mechanism is at play. Consequently the theory can account arbitrarily for any kind of behaviour under any set of circumstances.

However, whenever Pavlov advances specific testable predictions about psychopathological behaviour, he is often brilliantly original and what is more, he is often correct. Much experimental work in the Soviet Union, which has been concerned with detailed testing of the truth of Pavlov's hypothesis has confirmed his predictions without necessarily being tied to all aspects of the theory. In this country for instance, Venables and Tizard found the predicted paradoxical effects in schizophrenics to visual, though not to auditory stimulation.

The book is very well translated. The reviewer greatly enjoyed the rich, authoritative, often almost poetic style of the writing, though others may find it at times somewhat too bombastic and flamboyant. Two concluding chapters by Popov and Rokhlin on Pavlovian theory are, though well informed, too dogmatic, uncritical and lacking in originality.

BEATE HERMELIN.

Physiological and Behavioral Aspects of Taste. Edited by Morley R. Kare and Bruce P. Halpern. London. Univ. Chicago Press. 1961. Pp. xv + 149. 48s.

This book is an edited transcript of a symposium held at Cornell University in June, 1960.

In an informative Preface, Pfaffmann highlights the need for an interdisciplinary approach to the central theme: "gustatory control of behaviour." The symposium is certainly a step in that direction, but it fails to come to grips with the problem of the motivational role of taste stimuli in behaviour. From time to time throughout the proceedings references are made to the fact that responses elicited by taste stimuli appear to influence food preferences and intake, but the implications behind these references are not discussed. The reviewer feels that this may be due to the implicit assumption, apparently held by most of the contributors, that the sensitivity of the taste mechanism is highly stable, so that the quality and intensity of the gustatory responses elicited by a given taste stimulus, remain essentially constant. If such were the case, gustatory control of behaviour would be rather limited and one would tend to look for factors outside the taste mechanism to explain the apparent nutritional wisdom so often exhibited by naive organisms. However, this implicit assumption is contrary to the growing body of evidence in the literature which clearly demonstrates that the Nervous System is capable of regulating its own sensory input, and to studies which strongly suggest that the sensitivity of the taste mechanism varies significantly with time of day, food intake, and certain deficiency states.

Nevertheless, most of the papers are informative and much of the discussion lively and pertinent. For example, Kare presents evidence of phylogenetic and ontogenetic differences in response to certain taste substances. Further evidence of species differences and

some difficulties of control in recording neural responses are discussed by Kitchell. Frommer presents fairly conclusive evidence of "Gustatory afferent responses in the thalamus" of the rat. "Methodology in preference testing" is discussed by Christensen, who also outlines an electronic preference tester which considerably reduces the time taken and most probably increases the accuracy in the recording of brief-exposure preference data. The complex nature of possible interactions between food intake and metabolic functioning is shown in the papers by Tepperman and Heggness. The review of "Research on taste in the Soviet Union" by Pick is quite informative especially with regard to the use of "unconditioned- and conditioned-response techniques."

The reviewer finds the inclusion of Pilgrim's study on "Interactions of supra-threshold taste stimuli" somewhat disquieting, because its rather obvious lack of control rates it well below the standard of the above papers. Since the influence of practice effects on subjective judgements of quality and intensity have been demonstrated often enough, one wonders why this study was accepted, as Pilgrim failed to control these factors and moreover based his results on only one judgement per subject.

To sum up, although this book contains some important contributions, it is rather disappointing due to its poor coverage of the central theme "gustatory control of behaviour."

ROY YENSEN.

The Perception of Causality. By A. Michotte. Translated by T. R. and E. M. Miles. Foreword by R. C. Oldfield. Methuen & Co., Ltd. London, 1963. Pp. xxii + 425. 45s. net.

Causalité, Permanence et Réalité Phénoménales. By A. Michotte and collaborators. Louvain: Publications Universitaires. 1962. Pp. 612. FB. 500.

It has long been an open secret that Professor and Mrs. Miles have been engaged in preparing an English translation of Michotte's *La Perception de la Causalité*, first published in 1946 and reviewed in this *Journal*, 1, 1948, 104-5. Those of us who have been keenly anticipating the result of their labours will not be disappointed. The translation is exceptionally sensitive and scholarly and is embellished with an admirable commentary by Professor Miles, in which he not only elucidates some difficult points in the text but adds some most useful comments of his own on Michotte's methods and the relation of his work to the views of Hume. Professor Michotte has added a long appendix on "Theory of Phenomenal Causality: New Perspectives," written expressly for this edition, and Professor Carolus Oldfield contributes a perceptive Foreword. This book is one of the very few psychological classics of our time.

The large volume of papers to mark the occasion of Professor Michotte's 80th birthday brings together much of the more recent experimental work on phenomenal causation and related problems. It is pleasant to be able to record that no fewer than four originally appeared in the pages of this *Journal*.

O. L. ZANGWILL.

Brett's History of Psychology. Edited and abridged by R. S. Peters. London, George Allen and Unwin Ltd.; New York, the Macmillan Company. Revised edition, 1962. Pp. 778. 60s.

The first edition of R. S. Peters' abridgement of Brett's *History* appeared in 1953. The revised edition contains an extra 36 pages. Dr. Peters has added his own expositions of Aristotle and of Hobbes; and the sections entitled *From Taine to Binet* and *Feeling and Emotion*, which were omitted in the first abridgement, have now been restored. The final chapter is renamed *Twentieth Century Theories*, and is enlarged by (*inter alia*) a new account of B. F. Skinner (for which a page or so on operationism has been sacrificed), additional material on Freud, and some remarks on explanation in psychology.

The 1953 edition of Brett received a lengthy notice in this *Journal*, 5, 185-6. All the sins of commission which the reviewer pointed out have been set right, and so have one or two of the sins of omission. The price has risen by 18s., but the print and the paper are better.

ALAN GAULD.

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Part 4

THE VISUAL DISCRIMINATION OF SHAPE BY OCTOPUS: THE EFFECTS OF STIMULUS SIZE

BY

N. S. SUTHERLAND and A. E. CARR

From the Institute of Experimental Psychology, University of Oxford

Octopuses were trained in a successive situation to discriminate between vertical and horizontal rectangles: Group *S* was trained with rectangles of side length 5×1 cm., Group *M* with 10×2 cm. rectangles, and Group *L* with 20×4 cm. rectangles. The larger the shapes used, the more readily were they discriminated, both in terms of speed of learning and of the asymptote of performance. After training, each group was given transfer tests with the two pairs of rectangles not used in training and with two further pairs of 2.5×0.5 cm. and 40×8 cm. The results can be summarized in three generalizations: (1) When the size of a shape is changed, performance is worse than on the original training shape. (2) The bigger the change in proportionate size, the less transfer is shown. (3) For corresponding changes of proportionate size, there is better transfer to larger shapes than to smaller. These generalizations are supported by data from earlier experiments on the question of transfer to different sized shapes: some of these data were reworked and are presented in detail here. The theoretical implications of the results are discussed.

If octopuses are trained to discriminate between two shapes, and are then presented with the same shapes in different sizes, they tend to show more transfer to larger than to smaller shapes. It has been found that when the transfer shapes have areas four times as great and four times as small as the originals, significantly less transfer occurs to the smaller shapes than to the larger (Sutherland, 1957—horizontal and vertical rectangles; Sutherland, 1960a—squares and rectangles; Sutherland, 1963a—"H" figures and crosses). When the transfer shapes have areas 2.25 times and 0.33 times those of the original shapes, transfer has also been found to be worse to small shapes than to large (Sutherland, 1959a—"M" and "V" shapes). When transfer shapes were only 1.4 and 0.6 times the area of the originals, no consistent difference in performance on the large and small transfer shapes appeared (Sutherland, 1958—squares and triangles; Sutherland, 1959b—circles, squares and triangles). It is evident that provided the differences between the areas of the transfer shapes and the originals are great enough, there is more transfer to large shapes than to small. One possible reason for this might be that larger shapes are themselves more readily discriminable by octopuses than are smaller. The major purpose of the present experiment was to determine whether this is correct by training different animals to discriminate between shapes of different sizes. In the experiments cited above, transfer to large shapes was sometimes found to be perfect even when they were four times the area of the originals: a second object of the present experiment was to discover whether transfer remains good when shapes much more than four times the area of the originals are shown.

EXPERIMENTAL METHODS

Only a brief summary of the methods used will be given here: for fuller details of the method of keeping and training animals *v. Sutherland, 1960a*.

Subjects

The subjects were 12 *Octopus vulgaris* Lamarck, each weighing between 250 and 500 gm. They were kept in individual tanks, with a home of bricks at one end.

Shapes

The shapes used were rectangles of five different sizes: 2.5×0.5 cm. (SS), 5×1 cm. (S), 10×2 cm. (M), 20×4 cm. (L), and 40×8 cm. (LL). These shapes will be referred to as "very small," "small," "medium," "large," and "very large" or by the letters given in brackets above. Two rectangles of each size were used—one horizontal, the other vertical: thus there were ten shapes in all. All shapes were cut from $\frac{1}{4}$ -in. thick white Perspex, and were attached to a transparent Perspex rod. The shapes were presented to the octopuses singly, and were shown at the far end of the tank from the animal's home. Whilst in the water, shapes were moved through a distance of about 3 cm. three times a second. Shapes were sometimes moved up and down, sometimes from side to side. In what follows, the words "vertical" and "horizontal" will always refer to the orientation of the rectangles, the words "up down" and "side to side" will be used to refer to the direction in which they were moved. Because the shapes were moved by hand, and because the Perspex rod on which they were mounted was slightly flexible, it is not possible to be certain that the side to side motion had exactly the same characteristics as the up down.

Pretraining

All animals were first trained to come out and attack a piece of sardine when this was presented on the end of a wire. They were then given ten days pretraining on six of the ten shapes: these were the large, medium and small horizontal and vertical rectangles. Twelve trials a day were given, six in the morning and six in the afternoon. On each day the shapes were moved up and down on one session and from side to side on the other: the direction of motion during each session alternated from day to day. Each shape appeared once in each session, but the order of presentation was varied randomly from session to session. Thus, animals were given 20 pretraining trials with each shape, ten of which involved up down motion, ten side to side motion. During pretraining, if an animal took a shape within 20 sec., it was rewarded with a piece of sardine and the shape was withdrawn: if an animal failed to attack within 20 sec., the shape was advanced down the tank, and if it still failed to attack, the experimenter lured it out by showing a piece of sardine near the shape. The purpose of pretraining with these six shapes was to make them all initially attractive to the octopuses. It was hoped that this would ensure that animals would continue to attack when these shapes were used in transfer tests, and it was felt that this would make the transfer tests a truer measure of discriminatory performance, since no discrimination can be shown if animals do not attack.

Training

Animals were split into three major groups for the purpose of training. All animals were trained to discriminate between vertical and horizontal rectangles, but one group (L) was trained with the large shapes, one (M) with the medium, and one (S) with the small. Within each group, two animals were trained with vertical rectangles positive, two with horizontal rectangles positive.

Two training sessions of ten trials each were given daily for 14 days. In each session, five positive and five negative trials were given in a randomized order: within sessions alternate trials were with up down and side to side movement. The order of positive and negative trials was such that within each session either two or three positive trials and two or three negative trials were given with side to side motion, and two or three with up down motion: over each day, half the positive trials and half the negative were given with side to side motion, and the other half with up down.

If an animal attacked a positive shape within 20 sec. of its presentation, it was rewarded with a sardine and the shape was withdrawn; if it failed to attack within 20 sec., the shape was withdrawn. On negative trials, the shape always remained in the water for 20 sec.: animals were given a 10-volt A.C. shock whenever they attacked. After the first few presentations, animals never attacked the negative shape more than once on any one trial.

Transfer

At the conclusion of training, animals received 80 transfer trials: each animal had been trained with two shapes, and each received ten transfer tests with each of the remaining eight shapes, five with side to side motion and five with up down. Transfer shapes were shown in a random order, except that each shape was shown once on successive blocks of eight trials. The transfer shapes were presented in exactly the same way as training shapes, but they were always removed from the tank when attacked or after 20 sec. if there was no attack. Attacks on transfer shapes were neither rewarded nor punished. Each transfer test was preceded by a retraining trial with one of the original shapes: 20 retraining trials were given a day and 16 transfer trials. Each group missed a transfer test once every five trials when one of the shapes on which it had been trained was being shown as a transfer shape to the other groups. The order in which different transfer shapes were presented was such that each shape succeeded a positive and a negative retraining trial an equal number of times.

RESULTS

Pretraining

The results of pretraining are presented in Table I: the figures show the number

TABLE I
ATTACKS DURING PRETRAINING

No. of attacks made on different shapes				No. of trials
Up Down		Side to Side		720
612		538		
V 335	H 277	V 240	H 298	360
All L: 392		All M: 394		All S: 364
				480

of times the different shapes in each direction of motion were attacked. A significantly higher proportion of attacks was made on shapes moved in an up down direction than on shapes moved from side to side (χ^2 for the difference between the two proportions is 230.1, d.f. = 1, $p < 0.001$). More attacks were made on shapes moved in the direction of their long axis than on shapes moved across their long axis (v. Table I, row 2, χ^2 for the significance of the difference is 35.4 with up down motion, 23.9 with side to side motion: in each case d.f. = 1, and $p < 0.001$). There was about the same number of attacks on medium as on large shapes, but significantly fewer on small shapes (χ^2 for the difference between the proportion of attacks on medium and on small shapes is 5.3, d.f. = 1, $p < 0.05$).

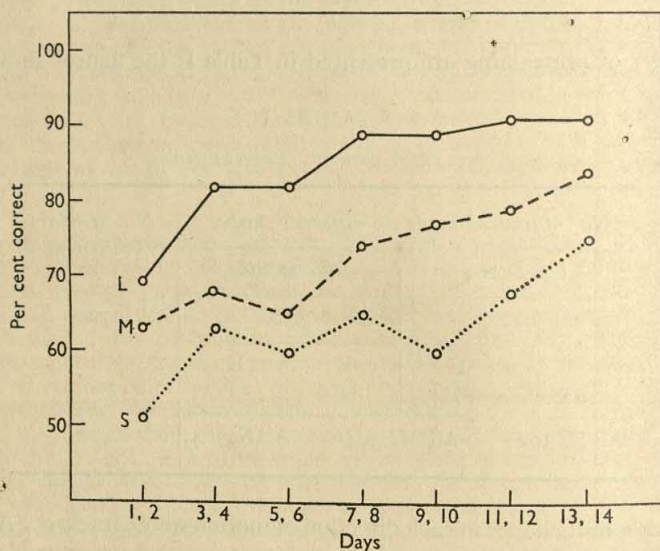
Training

Figure 1 presents a graph of learning: it is clear that animals trained on large shapes learned quickest, and those trained on small shapes slowest. The differences between the numbers of correct responses made by the three groups over all 14 days of training were assessed by means of t tests: the results were significant in all three cases. Thus, for $L, M t = 4.0$ (d.f. = 6, $p < 0.01$); for $M, S t = 3.0$ (d.f. = 6, $p < 0.05$); for $L, S t = 7.1$ (d.f. = 6, $p < 0.001$). We are thus entitled to conclude that for the shapes used in this experiment and within the size range here used, the larger the shapes the more readily discriminable they are to octopuses. Since it was shown in

pretraining that the medium-sized shapes were as attractive to animals as the large, the difference in rates of learning was not due to large shapes being attacked more often than the others, thus giving animals a greater opportunity to learn. On the first day of training all groups made about the same total number of attacks $L = 62$, $M = 72$, $S = 58$; subsequently more attacks were made by the L group than by the M , and more by the M group than by the S . These later differences in total attacks made by the different groups were presumably due to the effects of the larger number of shocks received by groups being trained on less discriminable shapes (cf. Sutherland, 1960b).

Animals performed differently when shapes were moved in different directions: this was to be expected (cf. Sutherland and Muntz, 1959) and will not receive detailed

FIGURE 1



comment here. It is sufficient to note that every single animal made more correct responses on trials on which the shapes were moved in the direction which made the positive shapes the preferred shape and the negative the less preferred, i.e. animals trained with the vertical shape positive did better when the shapes were moved in an up down direction than they did when the shapes were moved from side to side, and vice versa for animals trained with the horizontal rectangle positive. If for each group the scores of all animals on trials with up down motion are summed and compared with the scores on trials with side to side motion, no significant differences emerge.

Transfer tests

The results of the transfer tests were treated in the following way: for each animal the number of attacks made on each transfer shape was computed. We then calculated for each group the number of attacks made on positive and on negative transfer shapes: a transfer shape was taken to be "positive" for a given animal when it was in the same orientation as its original positive rectangle, and "negative" when in the same orientation as the original negative. The results are summarized in Table II.

TABLE II
TRANSFER TESTS

Shapes	S			M			L				
	Attacks on positive	Attacks on negative	Correct responses Trials %	Attacks on positive All attacks %	Attacks on negative	Correct responses Trials %	Attacks on positive All attacks %	Attacks on positive	Attacks on negative	Correct responses Trials %	Attacks on positive All attacks %
SS	8	2	58	80	7	54	59	13	4	61	76
S	13	2	65	87	10	65	69	17	10	59	63
M	25	12	66	68	2	88	95	29	7	78	81
L	30	16	68	65	12	75	73	33	1	91	98
LL	19	19	50	50	18	56	56	33	1	90	97
Total attacks on transfer trials	182	49	—	—	87	47	—	92	22	—	—

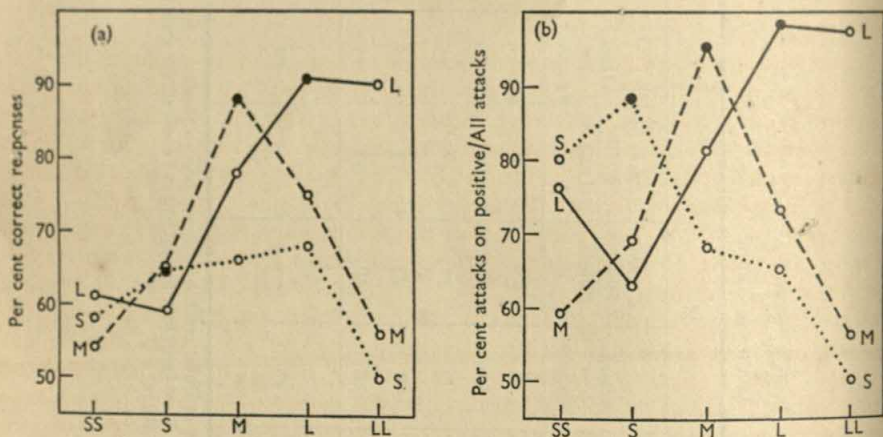
Figures in italics represent the average performance on the original training shapes per 40 trials.

The percentage figures in italics are based on the raw data: they therefore do not always correspond exactly with the figures for average number of attacks made since the latter figures are rounded to whole numbers.

The first two columns for each group give the number of attacks made (out of 40 trials) on positive and negative transfer shapes. Figures in *italics* indicate the average number of attacks made per 40 trials during the retraining trials on the original shapes which were interspersed with transfer test trials. It will be seen from the bottom row of Table II that animals trained on small shapes made about as many attacks during transfer tests as either of the other groups: thus poor discrimination during transfer tests by this group cannot be ascribed to failure to attack.

Figure 2 (a) presents the results graphically: three trends are apparent, and appear in the results for all groups. (1) Performance on the original shapes tends to be better than on any of the transfer shapes: the only exception to this is that Group *S* scores slightly and not significantly better on shapes *M* and *L* than on its original shapes (*S*). (2) The bigger the difference in size between the transfer shapes and the originals the

FIGURE 2



worse animals perform in terms of "correct" responses. Only two points on the graph (Group *S*, shape *L*, and Group *L*, shape *SS*) fail to conform to this generalization. In neither case is performance on the shapes cited significantly better than performance on shapes nearer in size to the originals. (3) Increasing the size of a shape by a certain ratio leads to better performance than reducing the size by the same ratio. Thus Group *L* performs better on the *LL* shape than the *M*; Group *M* performs better on the *L* shape than the *S*, and better on *LL* than *SS*; and Group *S* performs better on *M* than *SS*.

There is, however, a difficulty in the interpretation of the results. The extreme right-hand column of Table II makes it clear that different sized shapes were being attacked with different frequencies: the tendency not to attack small shapes which was revealed in pretraining shows up even more clearly in the results of transfer tests. We must consider whether some of the transfer test results are due not simply to failure to transfer a discrimination but to the decreasing attractiveness of smaller shapes. Thus, the group trained on large rectangles only attacked the *SS* shapes 17 times out of 80 trials: even had all attacks been made on the positive shape, this group could only have scored 85 per cent. correct with this number of attacks, i.e. less than it scored during retraining with the original shapes (cf. Sutherland, 1960b). We have therefore re-calculated the results in another way in order to eliminate the effects of the absolute number of attacks made. For each pair of shapes the percentage

of attacks on the positive shape out of all attacks was calculated, and the results are shown graphically in Figure 2 (b) (cf. Table II). Using this second measure of transfer it transpires that: (1) All groups discriminate better between the shapes on which they were originally trained than between any other pair. (2) There is now only one exception to the rule that the bigger the difference in size of transfer and test shape the lower the transfer: Group *L* on this measure of discrimination scores better on *SS* shapes than on *S*. (3) Except in the case of Group *L*, the differences between the amount of transfer to large and small shapes are largely eliminated. Thus, on this measure of performance, Group *M* only scores slightly higher on *L* than *S*, and it scores slightly lower on *LL* than *SS*; Group *S* scores slightly lower on *M* than *SS*. These reversals of this trend could be due to chance since there were very few attacks made on the smaller shapes. Further evidence on this question will be presented below.

DISCUSSION

Preferences

In both pretraining and training it was found that animals are more likely to attack shapes moved along their long axis than shapes moved across their long axis: this result merely corroborates earlier findings (Sutherland and Muntz, 1959). The finding that animals discriminate as well with side to side motion as with up down motion, provided preferences are controlled by training different animals with different shapes positive, also corroborates an earlier result (Sutherland, 1960c; 1961a). One of the present results is new, namely, that the medium and large sized shapes used are more attractive to octopuses than the small shapes. This was shown in pretraining, and is confirmed by the transfer test results: these also suggest that the very small rectangle may be even less attractive than the small, and it is possible that the very large rectangle is slightly less attractive than the large. The results with *SS* and *LL* shapes must, however, be interpreted with caution, since they were not presented in pretraining, and the reduced number of attacks made on these shapes may have arisen because animals had not learned to attack them in the experimental situation. Although the finding that large shapes are more likely to be attacked than small receives comment here for the first time, it is fully confirmed by earlier data collected by Sutherland. Table III shows the results of calculations based on the data of four previous experiments in which animals were tested with figures at least twice as large or as small as the originals. It will be seen from the left-hand columns that in every case animals attacked the large transfer shapes on a higher proportion of trials than the small. The training shapes used in these earlier experiments were all the same areal size as or slightly larger than those used in the present experiment. Thus, the present result has considerable generality in that it applies to a variety of other shapes.

The fact that in the present experiment the medium and large rectangles were attacked more frequently than the small is particularly striking, since the pieces of sardines on which animals were fed in the laboratory had a smaller surface area than the small shape. This suggests that animals' preferences for large shapes exist before they enter the laboratory. The octopus's staple diet in the Bay of Naples is probably crabs, and a large crab would present about the same surface area to an octopus as would the medium rectangle. However, hungry animals will attack much larger food objects, e.g. another octopus.

Discriminability of different sized shapes

The main hypothesis to be tested was that larger shapes would be more readily discriminable than smaller to the octopus: this was fully confirmed by the results of

initial training. Parriss and Young (1962) using similar shapes to those used in the present experiment also found this. Dodwell (1957) discovered that shapes of 12.5 sq. in. were more readily discriminated by rats than shapes of 2.25 sq. in., and Warren (1953) showed that the discriminability of shapes for macaque monkeys increased as their area was increased from 0.9 sq. in. to 5.4 sq. in. Thus, the present finding is in line with other results both on the octopus and on other species. It is worth considering briefly some possible explanations of this finding.

(1) The result might be due to poor visual acuity. This is extremely unlikely, since Sutherland (1963*b*) has found that octopuses readily learn to discriminate between stripes of 1.5 mm. and 3 mm. breadth.

(2) Although it has been shown that the result is not due to a greater tendency to attack larger shapes, it may be that in some way larger shapes are more attention compelling or more readily separable from their background. If this were so, more accurate representations of large shapes than of small might be set up in the nervous system because a greater arousal level would be produced by the large shapes.

(3) Young (in preparation) suggests tentatively that more cells may be "typed" on each presentation of a large shape than of a small. The difference between this hypothesis and hypothesis (2) is that (2) involves higher levels of activity but not necessarily more cells when larger shapes are used, (3) involves more cells but not necessarily higher levels of activity. There are two difficulties with this hypothesis: (a) Whilst *different* cells might well be involved when larger shapes are used, it is not obvious that the larger shapes will necessarily involve *more* cells in the central analysing system, though one might expect cells with larger dendrites to be involved (cf. Young, 1960). (b) The hypothesis would account for slower learning of a discrimination, but it does not make clear why the same asymptote of learning is not reached with different sized shapes. The latter objection applies but with slightly lesser force to hypothesis (2). There is little doubt that by the end of the present experiment all groups were performing approximately at asymptote since the scores on retraining over the five days of transfer tests remain very constant.

(4) The results of transfer tests suggest that the animals are learning the discrimination in terms of some sort of ratio between vertical and horizontal extents or between horizontal or vertical extent and the square root of the area of the shape (cf. Sutherland, 1960*a*; 1961*a*). Thus, the animals trained on large shapes treat the small horizontal rectangle as equivalent to the original horizontal, although its absolute horizontal extent is about the same as that of the large vertical. It may be that there is considerable neural noise in the central system for computing ratios, and that the noise has a proportionately greater effect on small shapes, thus masking to some extent the 25-1 difference between the ratios of the two shapes. This noise would not affect acuity if it occurred at the stage of the nervous system which computes ratios.

It must be admitted that it is not easy to think of differential behavioural predictions which would enable us to test between the last three hypotheses. It may be, of course, that all three factors are at work. If (2) is correct, then isolating the shape more clearly from the background should help the discrimination of small shapes, e.g. by having an even black background instead of the slightly mottled greyish background against which shapes were shown in the present experiment. All three explanations would predict that if a reduplicated pattern of vertical rectangles were shown against a reduplicated pattern of horizontal rectangles, discrimination should be considerably improved and experiments of this sort are needed to provide further evidence on which to base explanations.

Nothing has been said in the above about whether the physical size or the retinal size of the shapes is important. Parriss and Young (1962) have obtained results suggesting that physical size may be a more important determinant of performance than retinal size, and this question will not be further discussed here.

Transfer effects

It will be remembered that we tried to establish three generalizations on the basis of transfer test results, namely: (1) Performance on all transfer shapes is worse than on the original training shapes. (2) The bigger the change in proportionate size between transfer shape and original the less transfer there is. (3) For corresponding proportionate changes in size, there is more transfer to large shapes than to small. (2) is not in serious doubt since it fits well with the results of both methods of measuring transfer. (1) fits completely with the results yielded by the second measure of transfer performance (*v.* Fig. 2*b*), and (3) fits well with the results yielded by the first measure but rather poorly with the second. It was argued that where points on Figures 2*a* and 2*b* failed to fit with these generalizations, the discrepancies were due either to experimental error or (in the case of the curves in Fig. 2*a*) to the animals' pre-existing tendency to attack medium and large shapes more than small. The first argument is rather tenuous: we therefore present in Table III some additional

TABLE III
RESULTS OF TRANSFER TESTS IN OTHER EXPERIMENTS

Reference	Shapes	No. of trials with each transfer shape	Attacks Trials, %			Correct responses Trials, %			Attacks on positive All attacks, %		
			Shapes			Shapes			Shapes		
			L	S	M	L	S	M	L	S	M
a. Sutherland (1957)	Horizontal and vertical rectangles	85	44	43	45	86	65	82	90	68	86
b. Sutherland (1959 <i>a</i>)	"M" and "V" shapes	100	30	22	35	74	62	76	90	77	87
c. Sutherland (1960 <i>a</i>)	Squares and rectangles	320	39	28	33	78	61	79	85	69	94
d. Sutherland (1963 <i>a</i>)	"H" and cross shapes	320	43	31	35	71	57	77	75	61	89

In all cases animals were originally trained on the medium shapes.

evidence based on the results of four other experiments. The figures given in Table III have been calculated from the protocols of these earlier experiments and much of the data on which they are based has not previously been published in full. It will be seen that, in all four experiments, by both performance measures, animals performed better on large transfer shapes than on small: this lends unequivocal support to generalization (3).

It will also be seen that in all four experiments performance on the small shapes was worse than on the original shapes: this is some support for generalization (1). However, in experiment (a) animals performed slightly better with the large transfer shapes than with the originals by both performance measures; they performed better by one measure and worse by the other in experiment (b). None of these differences is significant, and in experiment (b) the change in size ratio was by a factor of 2.25—not 4 as in the other experiments. By both measures, animals performed better on the original shapes than on larger shapes in experiments (c) and (d): the difference in scores is significant at better than the 0.01 level of confidence in three cases out of four (there are two performance measures for each experiment). Since the data of experiments (c) and (d) are based on many more trials than those of (a) and (b), it seems safer to rely on the results of the second two experiments, and these provide further confirmation of generalization (1). It should be added that Parriss and Young's (1962) findings support all three generalizations and that a variety of experiments on other species has revealed worse transfer to small than to large shapes (for a review of the literature *v.* Sutherland, 1961b).

We now attempt a tentative explanation of the three generalizations. We would expect performance on small shapes to be worse than performance on large (generalization 3), because smaller shapes are less discriminable from one another than are larger. Performance will be worse on larger shapes than on the originals (generalization 1) only if one of the dimensions being analysed is that of absolute size: if animals were merely analysing a ratio (e.g. height to breadth, or height to square root of area), performance on larger shapes should be as good as on the originals, since changing the area of the shape does not alter this ratio. On the other hand, for transfer to occur at all in some cases, a ratio must be being analysed: otherwise we cannot explain how an animal trained with a 5×1 cm. vertical rectangle positive treats a 20×4 cm. horizontal rectangle in the same way as it did the original negative figure, although the height of the horizontal transfer rectangle is almost the same as the height of the original vertical positive rectangle. The idea that the primary dimension of the shapes being analysed is a ratio, but that some analysis of absolute extents is taking place, is supported by other data (Sutherland, 1961a). For the same reasons we might expect that the larger the difference in dimensions between original and transfer shape (generalization 2), the worse transfer will be: the larger this difference, the lower is the similarity between transfer shape and original on one of the dimensions under analysis, namely, that of absolute horizontal or vertical extent.

One problem remains. Performance on a learned discrimination must involve (at least) two processes: (a) the establishment of representations of the discriminanda in the nervous system; (b) a mechanism for comparing the input on each trial with the established representations. We can ask whether the poorer performance with smaller shapes is due to a difficulty in establishing representations for these shapes or whether it is due to a difficulty in comparing the input from small shapes with the established representations. If the process of establishing representations consists mainly of learning which of a number of pre-existing discriminatory mechanisms to switch in (as suggested by Sutherland, 1959c, and by non-continuity theory in general), then there is no reason to suppose that the representations of smaller shapes would be worse than those of larger shapes after learning had taken place. The asymptote of performance might still be lower because of a difficulty in process (b). There is one interesting prediction to which this line of reasoning leads. If the difficulty in discriminating small shapes is due only to a deficit in process (b), it follows that if we take two groups of animals and train them to asymptote one on small shapes, the other on large, then performance during transfer to an intermediate sized shape should

be the same for both groups. In the present experiment these conditions are fulfilled for the *S* and *L* groups when they are transferred to shape *M*. Group *L*, in fact, performed better on transfer shape *M* than Group *S*, but the difference was not significant. Hence the data of this experiment do not enable us to decide whether the difficulty animals have in discriminating small shapes is to be explained by inadequate functioning of process (a) or (b) or both. This constitutes an interesting problem for further research.

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REVERSAL LEARNING IN OCTOPUS VULGARIS LAMARCK WITH AND WITHOUT IRRELEVANT CUES

BY

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Three groups of eight octopuses each were trained on a brightness discrimination and its reversal. Within each group half the animals were overtrained on the original problem and half were not. For Group I the discrimination was a simple one, for the other two groups irrelevant cues were introduced (for Group II position and for Group III orientation).

It was found that the overtrained animals learnt the reversal significantly faster than the non-overtrained only where irrelevant cues were present (i.e. only in Groups II and III). The results are discussed in relation to various theories of discrimination reversal.

INTRODUCTION

A number of investigators (e.g. Reid, 1953; Pubols, 1956) have shown that for rats, overtraining on a simple discrimination facilitates subsequent reversal learning. A previous experiment by one of the authors (Mackintosh, J., 1962) failed to demonstrate any such effect in octopuses: after learning a horizontal-vertical discrimination by the successive training method, animals were overtrained by 20 or 60 trials, and then, after 20 pretraining trials to the original negative, learnt the reversal. There was no difference in the speed of learning between the overtrained and non-overtrained. There are several possible explanations of this finding:—

- (1) Overtraining has an effect on reversal learning only in certain animals—perhaps only in mammals.
- (2) The difference in amount of overtraining between the two groups was not enough to show an effect.
- (3) The animals were only given a fixed number of reversal pretraining trials. It is possible that overtrained animals are initially less likely to attack the negative shape; in which case they would begin reversal learning at a disadvantage.
- (4) The discrimination used in this first experiment was too simple, in that there were no irrelevant cues involved. In the ordinary simultaneous situation the positive and negative stimuli are placed randomly, either on the right or the left; and place may be said to constitute an irrelevant cue, in the sense that the animal may always select the right-hand stimulus, but by doing so never scores above 50 per cent. It should be noted that the majority of investigators of reversal learning in the rat have used a simultaneous brightness discrimination, so that in most cases it has only been shown that overtraining has an effect in situations containing irrelevant cues. It is possible to introduce irrelevant cues other than position: for example, in a brightness discrimination where the positive and negative cues are black and white rectangles, the positive may be vertical and the negative horizontal on half the trials, and vice versa on the other half. Orientation then becomes an irrelevant cue, and an animal always selecting the vertical rectangle regardless of brightness will only score 50 per cent.

The importance of irrelevant cues may be explained by the following theory of reversal learning (see Sutherland, 1959; Mackintosh, N. J., 1962): discrimination learning is a two-stage process; first an analysing mechanism specific to a given stimulus dimension is switched in; secondly approach and avoidance responses are attached to the different outputs of the analyser (representing the positive and negative stimuli). The effect of overtraining will be to strengthen the first process, so that when the value of the two stimuli is reversed, overtrained animals will continue to switch in the appropriate analyser, and will only have to learn to reverse the responses attached to the outputs. Non-overtrained animals will not have learnt so thoroughly to switch in the appropriate analyser, and, when their responses stop leading to reward, may switch in an inappropriate analyser, thus learning little about the reward values of the relevant cues; their responses, in fact, are likely to be controlled by irrelevant aspects of the situation. Where there are no deliberately introduced irrelevant cues, non-overtrained animals will not be greatly hindered (though it is likely that irrelevant cues exist in even the simplest of discrimination situations); where such cues are introduced, the effect of overtraining should be considerably greater.

The main purpose of the present experiment was to test this hypothesis. Octopuses were trained on a brightness discrimination: half were overtrained before being reversed and half were not. There were three main groups: for the first group no irrelevant cues were introduced: for the second group position, and for the third group orientation acted as irrelevant cues. It was predicted that overtraining would have little effect on reversal learning for the first group, but considerable effect for the second and third groups.

Since in Mackintosh's original experiment the difference in the amount of overtraining may not have been enough (reason (2) above), more overtraining was given here—either 75 or 100 trials.

To investigate reason (3) above, reversal pretraining to a criterion was given to the first and third groups (given successive training), and for a fixed number of trials to the second group (given simultaneous training).

METHOD

Experimental design

There were three groups each of eight animals. Within each group four animals were overtrained, four not overtrained, and of these half were trained with black, half with white, positive. Group I was trained by the successive method to discriminate between a black and a white vertical rectangle. Group II was trained by the simultaneous method on the same figures. Group III was trained by the successive method to discriminate between black and white rectangles, the orientation of which was either horizontal or vertical.

The shapes used were 10×2 cm. black or white rectangles cut from $\frac{1}{4}$ in. perspex sheet.

Subjects

The subjects were 24 *Octopus vulgaris* Lamarck obtained from the Bay of Naples. They weighed between 250 and 500 gm. on entering the laboratory. They were kept in individual tanks, filled with continuously circulating sea water, at one end of which was a brick "home."

Acclimatization

Subjects were given two to three days to adapt to the tanks, after which they were trained to attack pieces of sardine attached to a prong until they attacked regularly within 5 sec.

Groups I and III

Pretraining. The positive shape was presented 10 times a day. If subjects failed to attack within 20 sec. a piece of sardine was shown and they were allowed to attack shape and bait together. Pretraining was continued until the shape was attacked within 20 sec. nine or ten times out of ten on any one day. For Group III there were two positive shapes, either a horizontal or a vertical rectangle, and they were presented alternately.

Training. The subjects were then trained on the successive brightness discrimination. There were 20 trials a day in two blocks of 10. Attack on the positive within 20 sec. was rewarded with a piece of sardine; attack on the negative within 20 sec. was punished by a 5-volt electric shock. Positive and negative trials were given in a random order; for Group III a further random order determined whether the shape shown on any trial was horizontal or vertical. Within each day there were an equal number of positive and negative trials, and for Group III the positive and negative were equally often horizontal and vertical. Training was continued to a criterion of 90 per cent. correct responses on any one day's trials, after which half the subjects received 100 overtraining trials, and half began reversal pretraining. (For further details of training octopuses see Sutherland, 1957.)

Reversal pretraining and training. Pretraining was given with the new positive (the old negative) in the same way as the original pretraining, until the same criterion of 9 out of 10 was reached. This was followed by reversal training. Again the procedure was the same as for original training, and the same criterion of learning (90 per cent. correct responses on any one day's trials) was used.

Group II

Apparatus. In the simultaneous situation, the shapes, on transparent perspex rods, were suspended in the water at the end of the tank near the experimenter, while the octopus sat in its home at the far end of the tank. Between the shapes was a moveable "Eternit" partition projection 30 cm. towards the centre of the tank. In order to keep the octopus at the far end while the shapes were being introduced, a transparent perspex screen was placed across the tank 30 cm. away from the home end of the tank. In the centre of the screen a 15 × 15 cm. door was cut, which could be opened by raising a transparent shutter. This ensured that when the octopus attacked it did so from a position mid-way between the two shapes.

Procedure. The procedure for each trial was as follows. First the perspex screen with the door lowered, and then the "Eternit" partition, were put into the tank. The two shapes were introduced simultaneously each mid-way between the partition and one side. A piece of sardine on a prong was placed near the door in the screen to attract the octopus's attention, unless it was already sitting close to the door. After a gap of 5 sec. to allow the octopus to see both shapes, the door was raised and the trial began. If the octopus showed no signs of attacking within approximately 30 sec. the shapes were moved up and down for a further 30 sec. If the octopus still refused to attack it was lured out through the door by a piece of sardine on a prong held in line with the partition. An attack was scored only when the octopus grasped one of the shapes—they were allowed to retrace and go to the other shape if they had only approached the first. They were rewarded with a piece of sardine for a correct response, but no shock was given for an incorrect response.

Positive pretraining. After acclimatisation 3 days' positive pretraining was given. On the first day the positive was presented alone without screen or partition, exactly as for successive training. On the second day the partition was introduced, and 10 trials were given with the shape alternately on the right or the left. On the third day both screen and partition were used and a further 10 trials were given.

Training. Ten trials were given a day in two blocks of five at 30 min. intervals. The positive was randomly on the left or right. A correction method of training was used; after an incorrect response the negative shape was removed, but the positive shape was left in until the octopus attacked it and was rewarded. Training continued to a criterion of 90 per cent. correct responses over two days. Subjects were then divided into two groups; half began reversal pretraining immediately, the remainder were first overtrained for 75 trials.

Reversal pretraining and training. Ten pretraining trials were given with the new positive (old negative), the procedure being the same as on the third day of original pretraining. The following day reversal training began and was continued to the same

90 per cent. criterion. Reversal training was the same as original training except that a non-correction method was used. If an animal attacked the negative shape, both shapes were immediately removed from the water and the trial ended. The reason for this change of procedure was that a correction training method tends to break up position habits, and the analysis of these was considered of interest.

RESULTS

The results are summarized in Table I. The measure given throughout is the number of errors to reach the criterion of learning (for reversal pretraining this means the number of failures to attack.) This measure was chosen as being more sensitive than the number of trials to criterion, since where the criterion is a percentage of correct responses over a day's trials, a difference of one error can mean a difference of 20 trials. The results for subjects trained in opposite directions (with either black or white positive) are combined, since an analysis of variance for original learning and reversal learning scores showed this effect to be not significant ($F = 2.4$, $p > 0.10$).

TABLE I
MEAN LEARNING SCORES DURING TRAINING AND REVERSAL

Group	Number of overtraining trials	Original learning	Reversal pretraining	Reversal learning	
		Errors to criterion	Errors to criterion	Errors to criterion	Blocks of 10 trials with responses to irrelevant cues
I B-W Successive	100	7.0	12.25	14.25	—
	0	8.75	20.0	21.0	—
II B-W Simultaneous	75	2.5	—	15.5	0.75
	0	2.75	—	39.5	3.25
III B-W Successive with H-V irrelevant	100	12.5	25.5	20.5	0
	0	9.75	32.5	37.75	3.5

Original learning

Within each main experimental group, the overtrained and non-overtrained subgroups were closely matched for learning scores. The largest difference—that within Group III—did not approach significance ($t = 0.7$, *d.f.* 6, $p > 0.10$). The difference between experimental groups was significant at the 1 per cent. level ($F = 7.1$, *d.f.* 2, 21). This largely reflects the very small number of errors made by Group II, trained by the simultaneous method.

Reversal

Pretraining. An analysis of variance was performed on the scores of subjects in Groups I and III. Although the differences appear considerable, the within group variance was extremely large, and no effects were significant. For both direction of training (black or white positive) and overtraining, $F < 1$; for the difference between Group I and Group III, $F = 2.5$, *d.f.* 1, 12, $p > 0.10$.

Learning. An analysis of variance for all groups showed that the effect of overtraining was significant at the 0.1 per cent. level ($F = 20.6$, *d.f.* 1, 19). A separate analysis performed on the scores of the two groups with irrelevant cues (Groups II and III) showed no difference between these groups ($F < 1$), and no interaction between groups and overtraining ($F < 1$). Accordingly, the scores of these two groups were combined to test the effect of the presence of irrelevant cues on reversal learning. On overtrained subjects the irrelevant cues had little effect ($F = 1.9$, *d.f.* 1, 9, $p > 0.10$); but on non-overtrained subjects the effect was significant at the 5 per cent. level ($F = 5.1$, *d.f.* 1, 9). For all subjects combined, the interaction between overtraining and the presence of irrelevant cues was not quite significant ($F = 3.5$, *d.f.* 1, 19, $0.10 > p > 0.05$).

As a direct measure of the effects of overtraining for each main group, *t*-tests were performed: for Group I, $t = 1.5$, *d.f.* 6, $p > 0.10$; for Group II, $t = 4.6$, *d.f.* 6, $p < 0.01$; for Group III, $t = 3.2$, *d.f.* 6, $p < 0.02$.

Overtraining significantly facilitated reversal learning only for the two groups with irrelevant cues. The presence of irrelevant cues impeded learning for non-overtrained subjects, but had no effect on overtrained subjects: overtraining thus had a slightly greater effect on reversal learning when irrelevant cues were present, although this effect was not quite significant.

Responses to irrelevant cues (Groups II and III). An arbitrary criterion was chosen for deciding whether, during reversal learning, subjects' responses were controlled by irrelevant cues. For Group III this criterion was a difference of 2 or more in the responses to the two irrelevant cues within a block of 10 trials. For Group II this criterion was doubled to a difference of 4 or more within a block of 10 trials, since in this simultaneous situation the number of responses to one irrelevant cue (position) determines the number of responses to the other.

The results of *t*-tests performed on these scores showed that in both Groups II and III, the non-overtrained subjects responded significantly more to the irrelevant cues than the overtrained subjects. For Group II, $t = 3.2$, *d.f.* 6, $p < 0.02$; for Group III, $t = 2.8$, $p < 0.05$.

DISCUSSION

It was suggested in the Introduction that one possible reason why Mackintosh found no effect of overtraining was that her subjects had not been reversal pretrained to a criterion. The results of this experiment rule out such an explanation. In the first place, all animals in Group II received 10 trials only of reversal pretraining, and an effect was found here. More important, however, is the finding that there was no significant difference in the number of trials to reach the reversal pretraining criterion between overtrained and non-overtrained groups. In fact, as in Mackintosh's experiment, the overtrained animals attacked the new positive rather more readily than the non-overtrained; in the present experiment this was reflected in the number of failures to attack before the criterion was reached; in the former experiment in the absolute number of attacks in 20 pretraining trials (the overtrained animals made an average of 9.2 attacks, the non-overtrained of 6.6 attacks—the difference was not significant ($t = 1.3$, $p > 0.10$)).

D'Amato and Jagoda (1960; 1961) have suggested in their discussion of reversal learning that the effect of overtraining is to reduce avoidance tendencies established early in training to the negative stimulus, so that overtrained animals are more willing, upon reversal, to approach the new positive. While the present results suggest that this occurs to a small extent, they also suggest that D'Amato and Jagoda's explanation of quicker reversal learning following overtraining is incomplete.

Since the effect of reversal pretraining to a criterion should be to equate the tendencies to attack the old negative for overtrained and non-overtrained groups, any difference between these groups should be confined to reversal pretraining, and should not appear in reversal learning itself.

That non-overtrained animals should have stronger avoidance tendencies to the old negative stimulus would also be expected from the finding of Young (1956) that the time during which shock on any figure never associated with food reduces the tendency of octopuses to attack it is of the order of several days.

The effect of irrelevant cues on reversal learning

In all but one respect the results completely confirm the predictions made in the Introduction. With irrelevant cues, overtraining had a significant effect on reversal learning; without irrelevant cues, a slight and not significant effect. At a descriptive level, the effect of overtraining in Groups II and III was due to the tendency of non-overtrained subjects to respond to the irrelevant cues during reversal significantly more than the overtrained subjects (an effect also shown for rats—*v. Mackintosh, N. J., 1962*). Overtrained subjects responded hardly at all to the irrelevant cues, and were not significantly hindered by their presence: non-overtrained subjects, however, learnt significantly slower when irrelevant cues were present. All this agrees well with Sutherland's theory: if the effect of overtraining is to ensure that the correct analyser is firmly switched in, then, even upon reversal, overtrained animals will continue to respond in terms of the relevant cues, whether or not the situation contains other, irrelevant cues; non-overtrained subjects, however, are likely to switch out the correct analyser when their responses no longer lead to reward, and will respond to any other cues that may be present—during which time they learn little about the reward values of the relevant cues. The deliberate introduction of irrelevant cues will tend to magnify this effect, thus increasing the time taken by non-overtrained subjects to learn the reversal.

The present results, however, do not establish that the introduction of irrelevant cues significantly *increases* the effect of overtraining: the interaction term only approached significance. But a further experiment by one of the authors (Mackintosh, N. J., 1963) with rats, designed to confirm and extend the present findings, does show a significant interaction between irrelevant cues and overtraining. It seems reasonable to conclude that for octopuses also, irrelevant cues do in fact increase the effect of overtraining.

Further evidence suggesting that as a result of overtraining on one set of cues, animals are less likely to respond in terms of a second set of cues present throughout training, is provided by an experiment by Bruner, Matter and Papanek (1955). They gave different groups of rats different amounts of training on a simultaneous brightness discrimination, and then gave a fixed number of trials with position also made relevant (i.e. now systematically correlated with reward). The amount learnt about position depended inversely on the amount of initial training. "This . . . suggests that the more thoroughly [the rats] had learnt to switch in the analysing mechanism which would detect the original cue, the less they switched in different analysing mechanisms when both cues were present and therefore the less they learned during this period about the second cue" (Sutherland, 1959, pp. 493-4).

While the results reported here and those of Bruner *et al.*'s experiment are consistent with Sutherland's theory, they can also be explained by other theories. For example, Harlow (1959) has suggested that the effect of overtraining is to eliminate "Error Factors," i.e. tendencies to respond in a systematically incorrect way. Clearly irrelevant cues as used in the present experiment act as a source

of such Error Factors; thus Harlow, too, would predict that the presence of irrelevant cues should increase the effect of overtraining. It should be possible however to decide between Harlow's and Sutherland's explanations. According to Sutherland, the reason why responses to irrelevant cues during reversal are reduced by overtraining, is because such training has firmly switched in the correct analyser; on Harlow's theory, it is because, during training, responses to the irrelevant cues did not lead to 100 per cent. reward. If the irrelevant cues were not introduced until after the original discrimination had been learnt, then on Harlow's theory overtraining should not diminish tendencies to respond to them, whereas, on Sutherland's theory, overtrained animals should still be less likely to respond to them (v. Mackintosh, N. J., 1963).

The present experiment provides the only example of a non-mammalian species showing improvement in reversal learning after overtraining. For example, slower reversal learning after overtraining has been found in Paradise Fish (Warren, 1960) and Leghorn chicks (Warren, Brookshire, Ball and Reynolds, 1960).

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THE EFFECT OF VARIOUS HETEROMODAL STIMULI ON VISUAL SENSITIVITY*

BY

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Ten experiments are reported. In each the effect of a heteromodal stimulus on visual sensitivity is investigated. With one exception all the stimuli produced a small increase in visual sensitivity. A number of possible explanations of intersensory facilitation are considered.

INTRODUCTION

As early as 1669 the Copenhagen anatomist Thomasius Bartholinus reported that partially deaf people could hear better in the light than in the dark. Many experiments have accumulated since this pioneering observation of intersensory facilitation to emphasize the relatedness of the sense modalities, and Hartmann (1935) provides a colourful introduction to the many variations on this theme under the title of "The Unity of the Senses." Ryan (1940) and Gilbert (1941) have published reviews of more limited areas, Gilbert's paper being particularly useful in the present context since it is limited to a critical survey of experiments demonstrating intersensory facilitation and inhibition. Harris's (1950) brief chapter repays attention as does the intimidating bibliography of the Russian work compiled by London (1954).

Although contradictions abound in the widely scattered literature one can agree with Harris (1950) that the weight of evidence favours the view that when one sense organ is stimulated the sensitivity of another is enhanced. Study of the negative reports in conjunction with those which demonstrate intersensory facilitation leads to three conclusions: first, that the heteromodal stimulus appears to increase sensitivity to the primary stimulus only if it does not attract attention to itself; second, that enhanced sensitivity to the primary stimulus may persist for at least 7 min. after the heteromodal stimulus has been withdrawn; and third, that there are marked individual differences which some would seek to link with personality variables. Some of the negative results can be understood in terms of one or more of these conclusions.

Explanations offered for the positive results vary. If, on the one hand, the investigator has studied the effect of stimulating one sense organ upon the sensitivity of another he has tended to look for specific anatomical connections between the two sensory systems. If, on the other hand, he has been impressed by the fact that simultaneous stimulation in any modality influences the sensitivity of any other sense organ then a general tonic effect produced by the heteromodal stimulus has been postulated, and no mention has been made of specific anatomical connections. Hypothetical hormones have been suggested as the agent of this effect, but spread of activity across the cerebral cortex from one primary sensory area to another has been the most widely accepted explanation.

* These experiments, which were carried out in the Psychology Department at Hull University, were first reported in a Ph.D. thesis submitted to the University of Reading in 1954.

METHOD

Apparatus

The width of a 6.5 cm. white line which just allowed it to be discriminated from a black ground was used as a measure of visual sensitivity. The variation in the width of the line was made by separating two matt black plates each measuring 8 by 3 cm. which were mounted side by side on a matt black metre square screen. A 6.5 cm. square on the screen behind the plates was painted white, and thus when the plates were separated the observer saw a white line the width of which was determined by the distance separating the plates. When the plates were closed he saw a black screen. The horizontal guides which held one of the plates stationary and between which the other could move laterally could just be discerned and they assisted the observer's fixation of the critical area of the screen.

The plates were separated smoothly and accurately by a simple mechanical system. A spring-loaded rod with a millimetre step-screw was screwed through a lug on the back of the fixed plate. Clockwise rotation of the rod caused its butt to bear on a lug fixed to the back of the moveable plate. Further rotation opened the plates against the tension of two independently adjustable springs. These ensured that during movement the plate remained vertical, and that it returned to the closed position when the butt of the rod was not in contact with the lug. A control knob and pointer moving over a 360 degree scale were incorporated, and with backlash reduced to a minimum displacements of $1/360$ mm. could be measured. The separation of the plates at the moment that the observer reported seeing the stimulus line was read directly from the dial, one degree on the scale representing a separation of $1/360$ mm. The control knob was manually operated in the first eight experiments. In experiments 9 and 10 a constant speed electric motor controlled by the observer was used to drive the control knob. To calibrate the apparatus the rod was turned until its butt was just in contact with the lug on the back of the moveable plate. In this position, with the plates in contact, the pointer was set at zero.

Procedure

General. The light adapted subject, who had attended the laboratory for a practice on the previous afternoon, was seated with his head held firmly in a head rest. His task was to press a report switch under his left hand, or the apparatus control switch in the case of experiments 9 and 10, when he first saw a white line appear against the black background. In experiments 1 and 2 his eyes were 10 m. from the test square which was evenly illuminated by shielded strip lighting to give a shadow-free illumination of 10 foot-candles. This will be known as Condition X. In the remaining eight experiments the subject's eyes were 3 m. from the test patch the illumination of which was reduced to 1 foot-candle. This will be known as Condition Y. The subject knew that the white line would appear between the two guides on the black background screen and he was asked to fixate this area. It was stressed that he should strive not to be distracted from the visual task, and to help him screens restricted his vision to the test area.

A modified method of limits was used. A rough threshold estimate was made. The apparatus was set at a constant value, 20 divisions on the scale, below threshold. After a variable time interval, ranging from between 1 and 5 sec. after a verbal signal, the experimenter began rotating the rod at 5 degrees per second. When the subject responded the experimenter noted the reading and returned the control knob to the starting position. In experiments 9 and 10 in which the subject controlled the apparatus, the experimenter set the dial at variable points between 20 and 40 units below threshold. On receiving a verbal signal, the subject switched on the motor

and switched it off when the line appeared. The reading was noted by the experimenter who then returned the dial to the next starting position. The starting position was determined by selecting a ticket from a box which contained 21 tickets numbered 20 to 40 inclusive.

During one session the subject made 30 threshold judgements. A few warming up trials were followed by 10 judgements recorded under standard conditions. This will be called the 1st Control Series. After 2 min. rest a series of 10 judgements were recorded while the heteromodal stimulus was being applied. This will be called the Stimulus Series. A 15-min. break during which the subject toyed with Kohs Blocks was followed by a further 10 judgements under standard conditions. This will be known as the 2nd Control Series. A rest of 1 min. was allowed after each 5th judgement. Catch trials were introduced into each session.

Experiment 1. Condition X. Olfactory

A bottle containing cotton wool soaked in oil of cloves was clamped 4 in. under the subject's nose during the Stimulus Series. A bottle containing plain cotton wool suitably coloured but odourless was substituted during the Control Series. The subject was asked to breathe normally through his nose and to concentrate on the visual task.

Experiment 2. Condition X. Nasal breathing

This experiment was suggested by Adrian's (1942) report that the frequency of the electrical activity in the pyriform area of the hedgehog is increased if air is blown up the nose and further increased if an odour is added to the air. For the Stimulus Series the subject was instructed to breathe through his nose, a sniff-like inspiration, and out through his mouth while making the judgement and to relax and breathe normally between judgements. During both Control Series he was asked to breathe through his mouth while making the judgements. All subjects needed some practice before the required breathing technique was mastered. This was done during the initial practice afternoon.

Experiment 3. Condition Y. Auditory

The subject wore earphones throughout the three series. During the Stimulus Series a 1,000 c.p.s. tone of 60 db. above threshold was fed into the earphones. The onset of the tone served as the ready signal, and the tone was switched off when the subject responded to the visual stimulus.

Experiment 4. Condition Y. Thermal

During the Stimulus Series the subject held his right hand in a tank of warm water maintained at a temperature of 45° C. The hand remained in the tank throughout the Stimulus Series.

Experiment 5. Condition Y. Gustatory

Gustatory stimulation was achieved by giving the subject two cubes of sugar. One was taken before the 1st and one before the 6th reading of the Stimulus Series.

Experiment 6. Condition Y. Proprioceptive

In a pilot experiment the subject was asked to maintain a steady muscular contraction of the right hand and arm by using a hand dynamometer. This proved to be a difficult task and visual sensitivity was decreased during the process. In the experiment reported here the task was simplified. The subject was given a squash

ball which he was asked to squeeze rhythmically while making his judgement. The procedure called for some preliminary practice since many subjects said that they found that the rhythmic squeezing with the right hand interfered with the response to the stimulus line which they made with the left hand. This procedure probably renders "Proprioceptive" too simple a label for the type of stimulation occurring in this experiment.

Experiment 7. Condition Y. Vestibular

This experiment was prompted by the reports (Spiegel, 1934, and Gerebtzoff, 1940) that vestibular stimulation provokes increased electrical activity throughout the cerebral cortex. Its design was influenced by Adrian's (1943) report that the modification in the discharge of the receptors of the semicircular canals persists for 20 sec. after rotation.

The subject was seated in a revolving chair with his head held in a head rest. During the Stimulus Series the subject was rotated 20 times in 10 sec. before the 1st, the 4th and the 7th judgements were made. A minute's rest was given after the 3rd and 6th judgements.

Experiment 8. Condition Y. Nasal breathing

This experiment repeats experiment 2 but under condition Y rather than condition X.

Experiment 9. Condition Y. Olfactory

This experiment repeats experiment 1 with two modifications. The experiment was performed under condition Y, and the apparatus was electrically operated and controlled by the subject himself.

Experiment 10. Condition Y. Control

The experimental procedure is identical with that employed in experiment 9 with one exception. A Control Series is substituted for the Stimulus Series. The control experiment was introduced to decide whether the position of the Stimulus Series in the experimental session was contributing to the results of the experiments.

SUBJECTS

The subjects were 108 secondary modern school children, 63 boys and 45 girls, whose ages ranged from 11 to 14 years. They were given no information about the experiment, but all were shown the apparatus and how it worked during the initial practice afternoon. Motivation was good; they missed half an hour's schooling and received sixpence a session for their pains. Children were chosen mainly for administrative convenience, but the fact that previous investigators (Werner, 1926, for example) have suggested that children, together with primitive peoples and psychopaths, are more susceptible to heteromodal influences also influenced the decision.

RESULTS

First, the performance of each subject was analysed individually. The difference between the means of the combined control threshold values (20 in all) and the 10 stimulus values was calculated for each subject and its significance tested using Fisher's "t" test. The percentage of subjects in each experiment who showed no significant threshold shift is shown in Table I. Excluding the final control experiment, on an average 17 per cent. of the subjects showed no significant change in

visual sensitivity. This figure compares with the 30 per cent. reported by Bornstein (1936), who suggests that these subjects correspond to Jaensch's disintegrates.

TABLE I

THE PERCENTAGE OF SUBJECTS IN EACH EXPERIMENT WHOSE VISUAL SENSITIVITY WAS NOT SIGNIFICANTLY INFLUENCED BY THE HETEROMODAL STIMULATION

<i>Experiment</i>	<i>N.</i>	<i>Percentage of subjects showing no threshold shift</i>
1. Olfactory (X)	105	8
2. Nasal breathing (X)	104	18
3. Auditory (Y)	52	35
4. Thermal (Y)	53	11
5. Gustatory (Y)	41	10
6. Proprioceptive (Y)	51	20
7. Vestibular (Y)	28	11
8. Nasal breathing (Y)	34	25
9. Olfactory (Y) electrical operation	12	16
10. Control (Y) electrical operation	11	100

Second, the standard deviation of the 20 control judgements for each subject was calculated as was the standard deviation of the 10 stimulus judgements. The means of these standard deviations for each experiment are shown in Table II. It was hoped that these data would contribute to an understanding of the negative results, and they are considered in the discussion.

TABLE II

THE MEANS OF THE STANDARD DEVIATION OF THE 20 CONTROL JUDGEMENTS AND OF THE STANDARD DEVIATION OF THE 10 STIMULUS JUDGEMENTS MADE BY EACH SUBJECT

<i>Experiment</i>	<i>N.</i>	<i>Mean S.D. of Control and Stimulus Series</i>	
		<i>Control</i>	<i>Stimulus</i>
1. Olfactory	105	7.1	7.3
2. Nasal breathing	104	6.9	6.7
3. Auditory	52	6.4	5.6
4. Thermal	53	6.5	5.6
5. Gustatory	41	6.4	5.1
6. Proprioceptive	51	6.5	6.0
7. Vestibular	28	5.4	3.8
8. Nasal breathing	34	6.6	6.0
9. Olfactory	12	5.0	6.9
10. Control	11	5.5	5.3

Third, a group analysis was made for each of the 10 experiments. The mean control threshold value and the mean stimulus threshold value for each experiment was calculated. The differences between these values were tested for significance. The results of these tests were given in Table III. Column A identifies the experiment. Column B gives the number of subjects. Column C gives the mean threshold values for the control and the stimulus conditions. Column D shows the actual

difference expressed as a percentage of the control mean. Column E gives the critical ratios. Excluding experiment 10, the control experiment, it will be seen that visual sensitivity, when measured under heteromodal stimulation conditions, is greater than when measured under control conditions. (A lower number in column C indicates a lower threshold.) In only one of these experiments, experiment 3, in which an auditory heteromodal stimulus was used, does the threshold shift fail to reach the 0.05 significance level. In the control experiment, experiment 10, there is clearly no difference between the 1st plus 3rd Control Series, and the 2nd Control Series which was substituted for the Stimulus Series of the other experiments.

TABLE III

A GROUP ANALYSIS OF THE RESULTS OF EACH EXPERIMENT

A. Experiment	B. N.	C.		D. Difference between means		E.	F.
		Mean threshold				C.R.	P.
		Control	Stimulus	Actual	%		
1. Olfactory ..	105	237.0	223.5	13.5	5.7	3.8	<0.001
2. Nasal breathing	104	244.5	234.0	10.5	4.3	2.6	<0.01
3. Auditory ..	52	156.0	151.5	4.5	2.9	1.0	>0.3
4. Thermal ..	53	156.5	147.0	9.5	6.1	4.2	<0.001
5. Gustatory ..	41	156.0	146.5	9.5	6.1	3.5	<0.001
6. Proprioceptive	51	155.5	147.0	8.5	5.5	4.0	<0.001
7. Vestibular ..	28	152.0	142.5	9.5	6.2	3.3	<0.001
8. Nasal breathing	34	145.0	137.0	8.0	5.3	2.1	<0.05
9. Olfactory ..	12	153.5	145.0	8.5	5.6	2.2	<0.05
		Control	Control				
10. Control.. ..	11	150.0	151.0	-1.0	-0.7	0.2	>0.8

The vast majority of responses to the catch trials occurred during the practice sessions. In all the experiments there were only 11 false positive responses. The subject was warned to respond only when he saw the white line and the series in which the false response occurred was re-started.

DISCUSSION

Positive results

To the economically minded the similar effect of different stimuli on visual sensitivity suggests that an explanation which is not limited to the properties of any single sensory system should first be examined. It is suggested that the heteromodal stimulus increases the excitability of the cerebral cortex. Such an explanation is not new. Hartman (1933a) referred to the general tonic effect of the heteromodal stimulus which brought the subject to a higher state of readiness, and the cortical irradiation theory which he and Kravkov (1934) proposed independently falls within this category. Bornstein (1936) claims that his "Hell" hormone, released by the action of the heteromodal stimulus, increases the activity level of the cerebral cortex, and Johnson (1920) was thinking along similar lines when he suggested that his hypothetical hormone increased muscle tone and the sensitivity of the synapse thus improving tactile sensitivity.

But theories in terms of cortical tonicity have been justly criticised by Ryan (1940) on the grounds that they are too vague; they do little more than to re-state the experimental results in terms which carry a physiological flavour. Harris (1950) too deprecates the lack of a direct neurological basis for the explanations. Even now these criticisms must be accepted rather than answered. It is true that a number of investigations of cortical dynamics (von Bonin, Garol, and McCulloch, 1942; Jasper, Ajmone-Marsan, and Stoltz, 1952, for example) have shown that occurrence of the type of cortico-cortical spread favoured by some of the early investigators is highly improbable. This makes the prophetic suggestions of the function of the brain stem reticular formation made by Herrick (1927) and in much more detail by Allen (1932) worthy of consideration. The pioneering experimental paper by Moruzzi and Magoun (1949) released a flood of investigations on this part of the central nervous system. Even with the warnings of Lashley (1952), Brain (1959) and particularly Brodal (1956) in mind it can still be claimed that no violence is done to what is known of the anatomical details or of the function of the system when it is suggested that it is through the good offices of the more caudal parts of the brain stem reticular formation that the various heteromodal stimuli exert their priming influence on the cerebral cortex.

Negative results

When considering the contradictory results in the intersensory stimulation field many early investigators have emphasized the importance of attending to the primary stimulus; maintaining the figure-ground relationship between primary and heteromodal stimulus as Thorne (1934) put it. Perhaps the experiment reported by Gregg and Brogden (1952) on the effect of simultaneous visual stimulation on auditory sensitivity underlines the importance of the control of attention most forcibly. In their experiment a significant decrease in auditory sensitivity was recorded when a light, gradually increasing in intensity, was introduced and the subject was asked to report its onset. A significant increase in auditory sensitivity occurred under the same conditions if no response to the light (the heteromodal stimulus) was demanded. The extension of this experiment by Thompson, Voss and Brogden (1958) also illustrates the same point although not in so decisive a manner.

If this explanation of the negative results is provisionally accepted some trace of the distracting effect of the heteromodal stimulus might be indexed by the size of the mean standard deviation of the stimulus series. A positive correlation between the number of subjects producing negative results (Table I), and the size of the mean S.D. (Table II) might be expected. In fact this is not so, the rank correlation coefficient is precisely zero. It might also be expected that the mean S.D.'s of the stimulus thresholds would be higher than the corresponding S.D.'s of the control thresholds. Contrary to these expectations it may be seen in Table II that the trend is in the opposite direction. The significance of this trend is weakened, however, by the fact that the S.D. of the control threshold is calculated from 20 threshold judgements with at least 20 min. separating the recording of the first 10 from the last 10. The S.D. of the stimulus series, on the other hand, is calculated from 10 threshold judgements made at one sitting. The only evidence for the distraction explanation is to be found in the introspections of the subjects, all of whom found the auditory stimulus more distracting than any of the others.

Negative results may also be due to extraneous stimulation occurring during the control series. Jasper (1936), when recording electro-cortical activity in the cat, found that although the animal could be perfectly relaxed cortical activation might

suddenly occur. He soon appreciated the reason for this unsolicited activation and dubbed it "the cat-box effect." It would be too optimistic to claim that such effects did not occur in some of the 500 or so experimental sessions reported here.

Other possibilities

The first explanation is that the heteromodal stimulus primes the cerebral cortex by way of the mechanism of crude awareness. But there are other possibilities.

The experimenter may have consistently influenced the data by his manipulation of the apparatus. Hartmann (1933b) had misgivings on this score, but was satisfied when he replicated his results using the constant method. The result of experiment 9 shows that significant differences in visual sensitivity did occur when the subject controlled the apparatus himself. Also, early in the series of experiments, the author asked two students to measure visual sensitivity under control conditions and under olfactory stimulation using the constant method. They did not know what result to expect and considered the task an exercise in the use of the constant method. The results showed a lower threshold for the olfactory condition.

A third possibility should also be considered. It has been said that the subjects were naive. This only means that they were not told that the various heteromodal stimuli would influence visual sensitivity. Hans, von Osten's horse, was naive as far as arithmetic was concerned but he developed an amazing sensitivity to subtle environmental cues appropriate reaction to which brought approval. It is possible that cues were available to the children which gave them greater encouragement and knowledge of results in some parts of the experiment than in others. The atmosphere of the experiments was kept as informal as the conditions already described would permit. During the breaks in the series, and between them, encouraging Pooh-like noises might be made, together with remarks such as "That is good" or "You are doing well." It may be that more effective encouragement was given during the stimulus series than during the control series. There is neither evidence nor recollection that this did in fact occur but it remains a possibility that such cues were more plentifully directed to the stimulus series. The importance of all this is evident when it is remembered that the Russian worker Schwartz (1957) has shown that it is possible to raise visual sensitivity by as much as 310 per cent. by appropriate verbal reinforcement, in his case knowledge of results. Greenspoon's (1950) grunts and nods of social approval also spell out a cautionary tale.

The results of the control experiment suggest that most weight should be placed on the first explanation of the results, but the other two are sufficiently plausible to warrant the use of a programming device in future experiments of this kind; a device which will allow less interaction between the sagacious subject and the garrulous experimenter.

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THE ORIGIN OF THE AUTOKINETIC EFFECT

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The effects of unbalanced fatigue of the ocular and neck musculature on the autokinetic effect are described. It is shown that after deviation of the eyes to an extreme position, fixation of a small light source with the eye in the central position gives rise to an immediate and pronounced autokinetic effect. This apparent movement is typically in the same meridian as that of the ocular deviation and is often, at all events at first, in the opposite direction. Similar, though less clear-cut, results are obtained after induction of unbalanced fatigue in the neck musculature. It is argued that the autokinetic effect observed under these conditions warrants treatment as a primary illusion of movement and does not involve a "framework" or standard of reference. In an appendix, a speculative model of the ocular control system and some preliminary observations are presented. It is concluded that the autokinetic effect is principally due to spontaneous minor fluctuations in the neural system which monitors the outward signals controlling eye movement.

In 1799, A. von Humboldt reported that a stationary point of light when steadily regarded in darkness appears to move (Carr, 1935). This phenomenon ("Sternschwanken") was long thought to be of physical origin and its psychological nature was not appreciated until very much later (Charpentier, 1887; Exner, 1896); we owe the term "autokinetic" to Aubert (1887). As ordinarily observed, the angular velocity of the apparent motion is from 2° to 3° per sec., and the total movement may amount to at least 30° . The effect is known to be influenced by a number of factors, e.g. the presence of additional lights in the field, an unexpected stimulus in another modality, the prevailing position of the eyes, and fatigue both local and general (Myers, 1925). The time of onset of autokinesis (although subject to marked individual variation) appears to depend on the area and illumination of the light source (though not its colour) and its direction is determined in some measure by the orientation of the shape of the light source (Honeyman, Cowper and Rose, 1946). The effect is also alleged to be sensitive to attitudinal factors affecting perceptual judgements (Sherif, 1936).

It was for long customary to ascribe the autokinetic effect to *eye-movements*; slow, involuntary drifts of the eyes, by causing the image of the light source to traverse the retina, being supposed to give rise to the impression of motion. Indeed, such a view is still upheld by Duke-Elder (1938). Although some individuals seem especially prone to involuntary eye-movement in the dark (Myers, 1925), the work of Guilford and Dallenbach (1928), using a photographic technique, has made clear that slow drifts of the eyes do not necessarily occur when the effect is witnessed and that there is in any case no consistent relation between the direction of such drifts and that of the autokinetic motion. This lack of correlation between eye-movements and autokinesis has recently been confirmed by Gregory (1959).

It was shown by Carr (1910) that the autokinetic effect is markedly influenced by *eye-position*. If, for example, the light source is observed with the eyes strongly deviated to one side, movement is commonly in the same direction as that of the ocular deviation. Further, Carr reported that if the eyes are held for a short time in an extreme position and then returned to the central position to fixate the light source, autokinetic movement tends to be at first in a direction opposite to that of the ocular deviation and to be followed by a movement in the reverse direction. The

first phase of this movement may be startlingly rapid and the light has indeed been described as "moving like a shooting star" (Myers, 1925). Lastly, Carr claimed that a volitionally induced eye-strain without actual deviation of the eyes (e.g. attempting to look to right or left without in fact altering fixation) may also affect the direction of autokinetic movement (Carr, 1910). More recently, Battersby *et al.* (1956) have shown that prolonged deviation of the head and trunk to one side may constrain apparent movement in the direction opposite to that of the postural shift. Although these various effects seem well attested it is important to note that individual differences are marked.

It is a matter for some surprise that these various effects of postural change on the autokinetic sensation have attracted so little notice. Although given some prominence by Carr (1910, 1935) in his "Innervatory theory" of space perception, they barely find mention in recent texts. This may be due in part to the influence of Gestalt theory, which in principle allowed little part to proprioceptive and "innervatory" factors in perception and endeavoured to account for autokinesis in terms of the lack of an appropriate "frame of reference" (Koffka, 1935, pp. 212-3). Although the "framework" concept has found useful application to the perception of motion both real and apparent (Duncker, 1929), there is reason to believe that the central "monitoring" of eye-movements plays a vital part in the perception of real motion (Gregory, 1958). If this be the case, it is not unduly fanciful to suppose that this process may undergo temporary derangement under conditions in which the oculomotor musculature is subjected to strain or fatigue. Indeed, Carr (1910, 1935) himself suggested that the autokinetic effect may well depend upon the continual minor "adjustments of innervation" which he believed necessary to inhibit random eye-movements. Although the explanation which we shall put forward below differs in some respects from that of Carr, we are at one with him in seeking to explain autokinesis in terms of the general properties of the ocular control systems involved in the perception of space and motion.

EXPERIMENTS

Experiment 1. Induction of apparent motion by unbalanced fatigue of the eye muscles

Selective fatigue was induced by requiring the subject to hold his eyes hard over in one of four extreme positions (up, down, right, left) for a period of 30 sec. He was then immediately required to fixate a small, dim, red light for 2 min. with eyes central. This light source was surrounded by a blue annulus to detect shifts of fixation, as described by Gregory (1959). During this period of fixation, the subject reported the direction of any apparent movement together with some indication of its velocity. For this purpose, he was asked to imagine a clock face with the red light as centre and to report directions of apparent movement in terms of hour numbers. Thus "12" would be a vertical upward movement of the light and "3" a horizontal movement to the right. The experiments were conducted in a sound-proof room and the reports recorded on a tape recorder in an adjacent room.

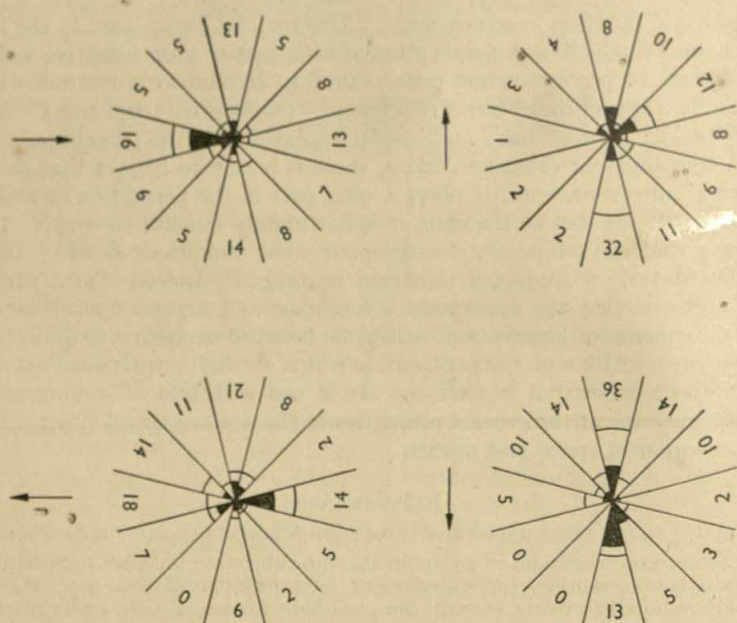
Results

The pooled results for 10 subjects (all young adults) are shown in Figure 1. In this "clock histogram," the twelve sectors represent the reported direction of apparent movement, the distance from the centre in any direction representing time. The shaded portions represent the directions of movement reported during the second 30 sec. of the 2 min. inspection period. The figures in the perimeter show total movement in the various directions over the whole inspection period. Periods in which no apparent movement was recorded are not indicated.

It will be seen from Figure 1 that the direction of movement over the first 30 sec. was commonly in a direction opposed to that of the imposed deviation of the gaze.

This is most clearly seen in the case of deviation to the left or right. It is noteworthy that the motion was typically reported as starting *immediately* the light source was fixated and not, as when observed under ordinary conditions, after an interval of 10 sec. or more. It was also said to be very rapid for several seconds; on one occasion, indeed, it continued in the same direction for as long as 93 sec. This initial movement often (though not always) reversed after a short interval and was succeeded by a relatively prolonged movement in the opposite direction. A typical individual record of the path of the autokinetic movement (as reconstructed from the subject's verbal reports) after deviation of the gaze upwards is shown in Figure 2. In general,

FIGURE 1



"Clock histograms" showing durations of apparent movement after eye strain. Direction of strain shown by the arrows: duration 30 sec. in each case. The black shading gives direction averaged over all subjects for the first 30 sec., the non-shaded portion over the second 30 sec. after eye strain.

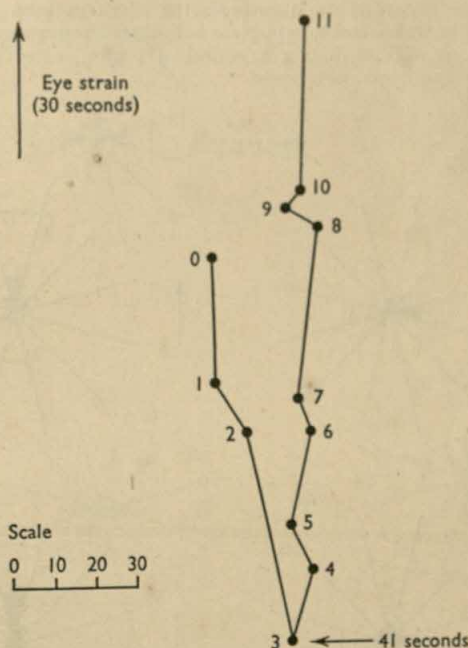
it was noteworthy that in spite of these very considerable excursions the light did not appear to change its position in space.

An analysis was made of the initial direction of the movement observed under the four conditions of the experiment. After deviation of the eyes to the right, initial movement was to the left in seven cases (clock position "nine" in five cases and "ten" in two cases); after deviation to the left, it was to the right in seven cases (clock position "three" in six cases and "two" in one case). In the case of "upward" and "downward" deviation, the results were more erratic but there was a comparable tendency for the direction of movement to be at first opposite to that of the ocular deviation.

The duration of autokinetic movement *in the same meridian* as that in which the eyes were deviated (i.e. left-right or up-down) was compared with its direction, in

the remaining meridians. Under all conditions and for every subject movement in the same meridian was significantly greater than movement in any other meridian. Over the first 30 sec., the movement was predominantly in the same meridian under every condition. (The lowest significance was 0.005, and this occurred only twice.) The same effect was still present over the second 30 sec. period, though here the differences were smaller and two conditions were not significant.

FIGURE 2



A typical "induced-autokinetic" movement after eye strain. This is derived from the reported directions of movement which were recorded on tape. It illustrates the typical reversal in direction occurring quite suddenly. (The clock histograms show the same type of data but averaged for all subjects. The reversal effect does not appear in the histograms due to variation in time of the reversal. Averaging masks the effect which is shown clearly here, though for one set of observations only).

Summary

(1) After deviation of the eyes to an extreme position (left, right, up or down), fixation of a small light source with the eyes in the central position gives rise to an immediate and well-marked autokinetic experience.

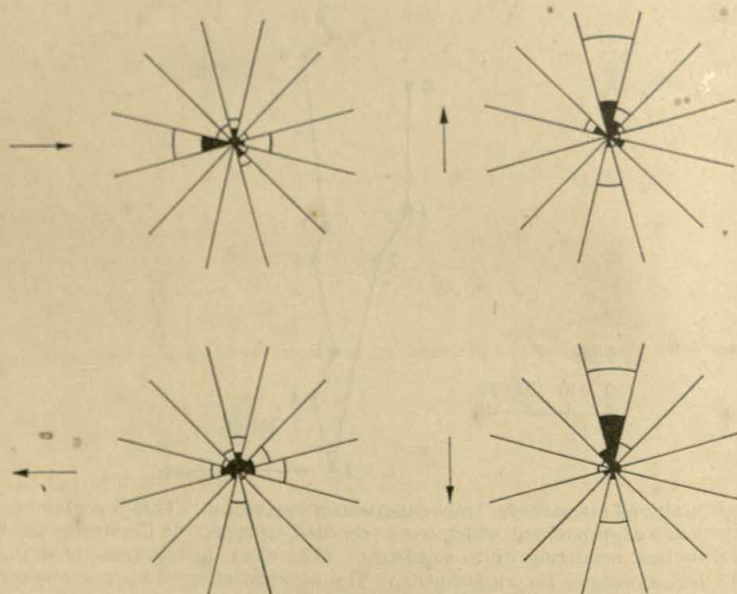
(2) The direction of autokinetic movement over the first 30 sec. is typically in the same meridian as that in which the eyes had been deviated and is commonly in the direction opposed to that of the deviation. The direction of movement is often, though not always, reversed after a short interval.

Experiment 2. Induction of apparent motion by unbalanced fatigue of the neck musculature

The procedure was essentially the same as in Experiment 1 except that in so far as tension was applied to the neck rather than to the ocular musculature, and the subjects were required to oppose the applied torque with an equal and opposite muscle tension.

A motor cycle crash helmet was modified by attaching to the crown a cross-shaped structure made from two aluminium bars arranged to lie in the sagittal and transverse planes of the head. Each bar projected 13 in. from the centre and 8 oz. weight could be hung from the back or front (at a distance of 12 in. from the centre) or from a string attached to one of the transverse arms (also at a distance of 12 in. from the centre) and passing over a pulley so arranged that a rotational torque was applied to the head round a vertical axis, the pulley being placed on a level with and behind the cross. (The torque round the horizontal axis also had a downward vertical component since no attempt was made to balance this by an equal upthrust from the opposite arm of the cross. Similarly, the torque round the vertical axis had a component tending to push the head backwards. These components were regarded as unimportant.) Instructions to the subject were as before, except in that he was asked to report on autokinetic movement during application of the torque (2 min.) as well as during a period of 4 min., after the weight had been removed. The same 10 subjects were used.

FIGURE 3



"Clock histograms" showing effect of torque applied to the head over a period of 60 sec. during the period of application of the torque.

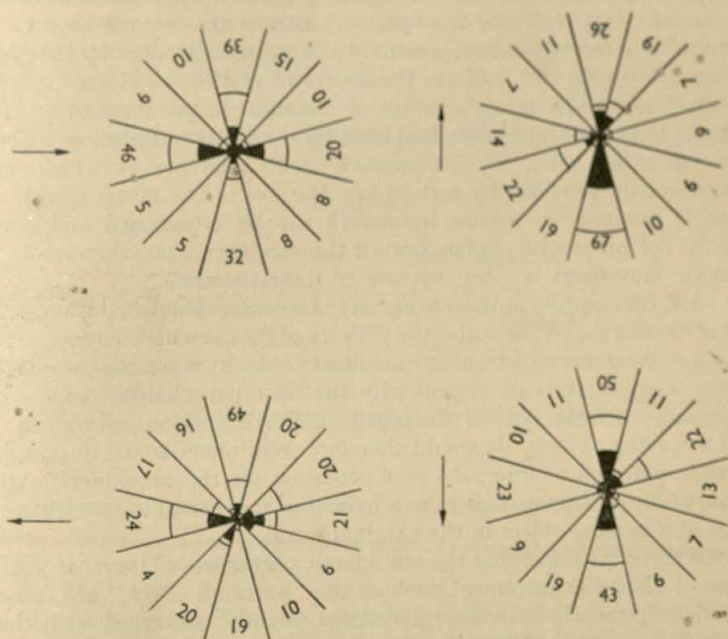
Results

The pooled results for direction of movement *during application of the torque* are shown in Figure 3. There is some slight indication that movement is most commonly in the direction opposite to that in which the torque had been applied.

The pooled results (10 subjects) for the first 60 sec. *after removal of the weight* are shown in Figure 4. Again, the shaded portions represent movement during the first 30 sec. and the unshaded portions movement during the second 30 sec. of the total observation period. The figures in the perimeter show total movement in the different sectors over a period of 2 min. after cessation of the torque. It will be seen that movement is predominantly in the same plane as that of application of the torque.

An analysis was made of the *initial direction* of movement observed under the four conditions. In the case of tension to right or left, no very clear tendency was noted for initial movement to be in a direction opposite to the side of tension; in the case of "upward" or "downward" tension, on the other hand, there was some tendency for initial movement to be in the direction opposite to that in which the torque had been applied.

FIGURE 4



"Clock histograms" showing the apparent movement following removal of the torque applied to the head as shown in Figure 3. (Shaded portion shows movement in first 30 sec., unshaded second 30 sec.) The figures in the perimeter give the total movement in each sector over a 2-min. period following the torque.

The duration of movement in the same plane as that in which the torque was applied for the first 30 sec. after removal of the weight was compared with the duration of movement in all other planes. The differences were highly significant for all subjects, the lowest degree of significance being 0.001. For the second 30 sec. period, only one difference failed to attain significance.

DISCUSSION

These results confirm earlier observations (Carr, 1910; Myers, 1925; Battersby *et al.*, 1956) indicating that the direction (and probably also the rate) of autokinetic movement are significantly affected by constraints imposed upon the oculomotor and neck musculature. As we have seen, the direction of apparent movement is typically in the same plane as that of the induced tension and, in its initial stages at least, most commonly in the opposite direction. It is also noteworthy that, under these conditions, the autokinetic movement begins immediately the light source is fixated and not after a short interval, as is usual under ordinary conditions of

observation. This initial movement was reported to be rapid and was often quite extensive. Curiously, it appeared to take place without an apparent shift in locus of the fixated light source, suggesting that the sensation of movement may, under certain circumstances, become divorced from the spatial properties of the environment.

The implications of the last observation appear to us of some importance. Hitherto, movement perception has been held to involve a relational process. "Movement," writes Carr (1910), "is relative. The fixed light must move in reference to something. This does not mean that the subject must necessarily be overtly conscious of the basal term and the space relation of the moving object. It does mean that the basal term must be represented at least in the background of experience. Its existence is necessary to indicate the source of motion." Myers (1925) adopts a very similar view, adducing the concept of "schema" as put forward by Head and Holmes (1911) to provide a physiological basis for the relational process. For Koffka (1935), on the other hand, the "framework" with reference to which motion is assessed is normally provided by certain key features of the visual world; if visual stimulation is minimal, no secure framework can be established and autokinesis may result. Yet no precise explanation of the incidence, rate, direction or extent of autokinetic movement is given by any of these theories.

In our view, these earlier authors are guilty of a conceptional confusion. Consider the speedometer of a car—it indicates the velocity of the car with reference to the road, but if it has a zero-error it could indicate an illusory velocity when the car is stationary. In the same way, there is no reason why the neural mechanism which indicates visual movement should not—if deranged—indicate spurious movement in the absence of a moving object. It would therefore seem unnecessary to postulate any "framework"—external or internal—as a condition for the autokinetic experience. It is necessary only to suppose that signals indicating movement are present in spite of the absence of a moving object in the external world.

We would therefore argue that the autokinetic phenomenon, together with certain other forms of illusional movement such as the "waterfall effect," are caused by a temporary derangement of the neural systems directly concerned with the visual registration of movement. These illusions should, we submit, be treated as primary illusions of movement and as in no wise due to faulty interpretation of the position of an object with regard to a "framework," perceptual or conceptual. The signals which engender the experience of motion do not necessarily depend upon movement in the stimuli any more than the experience of colour is necessarily dependent upon the presence of light of an appropriate wavelength. Indeed, illusions of movement are in no whit less primary than negative after-images. Such illusions, however, should be sharply distinguished from those involving figure-background relationships, as in the various forms of induced motion (Duncker, 1929), where the concept of a spatial framework is quite properly applicable.

APPENDIX

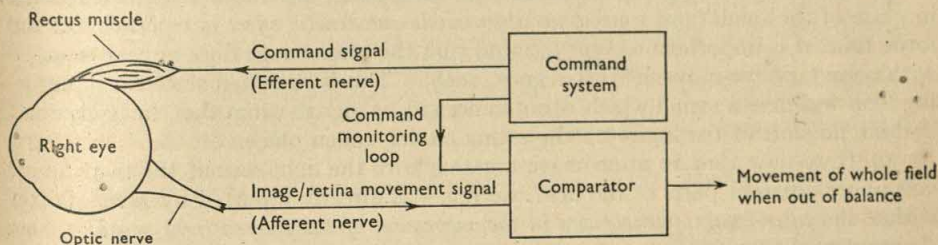
A SPECULATIVE MODEL OF THE OCULAR CONTROL SYSTEM AND SOME PRELIMINARY OBSERVATIONS

By R. L. GREGORY

An important problem in connection with apparent movement is why it does not occur with voluntary eye-movements, which produce shift of the retinal image without shift of the visual world. The explanation most commonly advanced is that the retinal movement signal arising from the shift of the image is cancelled by a

signal indicating that an eye movement has taken place. Although proprioception is now known to play a part in controlling the position of the eye (Brindley and Merton, 1960; Merton, 1961), there is little or nothing to suggest that it plays a significant role in the origin of apparent movement or in maintaining the stability of the visual world. The important factor here would seem to be a monitoring of the "command" signal to the eye-muscles, as originally suggested by Helmholtz (1924, 3, 242-68). His argument has been further developed by Gregory (1958) who has distinguished an "image/retina" system and an "eye/head" system, both of which are involved in maintaining the stability of the visual world. When the image of a stationary object moves across the retina (as with a voluntary eye-movement), signals from the "image/retina" system are cancelled by equal and opposite signals from the "eye/head" system, with resultant stability of the perceived environment. Gregory further suggested that certain forms of apparent visual

FIGURE 5



A diagram of the hypothetical "outflow" eye movement control system suggested by Helmholtz.

motion, in particular the "waterfall illusion," might be attributed to a process of adaptation within the "image/retina" system. A more detailed statement of the argument is given elsewhere (Gregory, 1958).

If we accept Gregory's position, it may be argued that the autokinetic phenomenon as observed under the experimental conditions described above is due primarily to changes of the nature of adaptation or fatigue located in the "eye/head" control system. What components of this system may be supposed to become adapted?

The schematic arrangement for eye-movement control suggested by Helmholtz is shown in Figure 5. As will be seen, it is conceived to possess three main components: (1) the oculo-motor system; (2) the monitoring system; (3) the central "command" system; (4) the cancelling system described in the figure as "comparator." The output of this provides the movement signal as a difference between the retinal and command signals. Theoretically at least, adaptation might occur in any of these components and it is pertinent to inquire whether its locus can be defined with greater precision.

Let us imagine a volitionally imposed strain on the ocular muscles induced by holding the eyes hard over, say to the extreme right. Now if the change is primarily in the musculature itself (e.g. muscular fatigue), it might be argued that stronger "command" signals would be required to maintain fixation immediately after the eyes have been returned to the central position. Now it is known that an intended but unaccomplished contraction of an eye muscle can effect a change in the localization of objects in the outside world (Myers, 1925; Kornmüller, 1930); hence an increase in "command" signal would be equivalent to a real movement to the right, i.e. in

the same direction as that of the imposed tension. This, however, is not generally found. It would seem, therefore, that the locus of the change is not in the eye muscles themselves.*

If the process of adaptation occurs within the "monitoring" system, it might be expected that the signal indicative of eye-movement would be reduced. This would be equivalent to a real movement to the left, that is to say, in the direction opposite to that in which the eyes had been deviated. As we have seen, this is the effect most commonly found in our experiments. Although it remains possible that changes might also take place in the "command" system, it is very difficult to see what form these might take or what effect they would have on the direction of perceived motion. Tentatively, therefore, we may conclude that the main focus of adaptation lies in the system responsible for monitoring eye-movements.

Preliminary observations. By changing the viewing conditions, additional information may be gained relevant to the above considerations. The following observations were made on nine subjects:

(1) If we repeat Experiment 1 using an after-image produced by a bright flash in place of the small light source, *no appreciable autokinetic effect is reported*. At the same time, it is important to bear in mind that the after-image does appear to move with voluntary eye-movements (Gregory, 1958). This is expected since the monitoring loop will give a signal which is not cancelled by signals from the "image/retina" system, no shift of the image on the retina having taken place.

(2) If we now view an after-image together with the light source, the two stimuli occupying different parts of the field, we find—as already reported by Myers (1925)—that the *after-image participates in the movement of the fixated light source*. This shows clearly that for induced autokinesis eye-position must remain constant, and supports the view that the locus of the effect is primarily in the system which monitors the "command" signals which institute eye-movements.

(3) It is possible to study the effects of previous eye-position (unbalanced strain) by the following very simple method: If the subject is required to keep a small dim blue light in central fixation (Gregory, 1959), any drifts of the eye in darkness can be noted since the blue patch is invisible when the image is projected strictly on the *fovea centralis* (Willmer, 1946). We find that (a) fixation cannot be continuously maintained, the blue patch frequently becoming visible as the eye drifts (although a blue annulus surrounding a red light can be kept continuously in central fixation); and (b) after the imposition of unbalanced strain, the eye may move either in the same or in the opposite direction to that of the strain, as judged by whether the blue light appears to the right or left of the centre of the field. Once it has appeared, the blue light may show autokinesis in either the same or the opposite direction to that of the strain, but once again in the same meridian.

These findings are in agreement with our interpretation of autokinesis as being due to fluctuating efficiency of the oculomotor system. Fluctuations in motor efficiency must be corrected by varying command signals in order to maintain fixation. These are wrongly taken to represent movement of the external light. This, we submit, is the origin of the autokinetic effect.

We are left with the following problem: Why do not these small unbalanced signals normally disturb the visual world?

* At the same time, we have some evidence that with shorter periods of induced muscle strain (less than 30 sec.) the initial movement tends to be in the *same* direction as that to which the eyes have been deviated. A similar effect was noted under some of the conditions studied by Carr (1910). It is possible, then, that some part of the effect may be attributed to peripheral changes in the oculomotor musculature.

We wish to thank Miss J. G. Wallace for much valuable help in the conduct of these experiments. She was supported on an M.R.C. grant.

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THE EFFECT OF REPEATED PRESENTATION OF ITEMS ON PAIRED ASSOCIATE LEARNING

BY

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An experimental design is outlined which allows the probability of an item being learned to be related to the number of presentations of that item. The results of an experiment of this design are considered in relation to the hypothesis that learning is a single trial occurrence. The results are found to support neither the single trial hypothesis nor the incremental hypothesis. A "dual factor" hypothesis is put forward and considered in relation to these and other experimental findings. The scoring procedures employed allow consideration of the response learning and the associative phases of paired associate learning. It is suggested that the response learning phase may be explained by the single trial hypothesis and the associative phase by the dual factor hypothesis.

INTRODUCTION

The results of several recent experiments have been interpreted as showing that learning takes place either on a single trial or not at all. The inference drawn from these results is that repetition is of importance in learning only in so far as it strengthens associations which have already been formed (i.e. on a single trial). This conclusion is of course in direct opposition to the more generally accepted principle that the probability of a given response increases in some manner with repeated association of the stimulus and the response.

The evidence for single-trial learning stems from experiments by Rock (1957), Rock and Heimer (1959), Clark, Lansford and Dallenbach (1960), Estes (1960) and Murdock and Babick (1961). Somewhat equivocal support for the hypothesis comes from results of experiments by Wogan and Waters (1959) and Reed and Riach (1960). On the other hand, Lockhead (1961) obtains results which run counter to the single-trial learning hypothesis. Most of the experiments designed to test this hypothesis have involved paired-associate learning (with a wide variety of materials), but results which are claimed as evidence for single-trial learning have also been obtained in the fields of free verbal recall (Estes, 1960; Murdock and Babick, 1961) and classical conditioning (Estes, 1960).

Most of the work in this field has involved minor variations of the experimental design originated by Rock (1957), who compared two groups of subjects on a paired-associate task. The control group learned a list of items to criterion according to the standard procedure of a recall test following each presentation of the list, with times, serial-order effects, etc., controlled. The experimental group was subject to this procedure in every respect, except that each pair that was not correctly associated on the recall test was replaced by a new pair randomly selected from a previously prepared pool. In other words, subjects in the experimental group were presented with pairs that were *either* new *or* already learned on each trial. Subjects in the control group did not suffer such a seeming disadvantage. The criterion used was one errorless recall of the list, and the median number of trials to criterion was found to be identical for both groups. Similar results have been obtained by the other workers cited above, despite the fact that these experiments were mainly conducted to control variables which might have favoured the experimental group in Rock's original study.

The conclusion drawn from these experiments is that repeated presentation of a given item does not facilitate learning of that item: the probability of a subject learning an item is not greater after, say, four presentations of that item than is the probability of him learning an item on a single trial. It may be noted that most later experiments of this type were carried out with the expectation of showing that Rock's results were due to some artefact concealed in his method or materials. Thus both Clark, Lansford and Dallenbach (1960), and Estes (1960), for instance, regret having to support the single-trial hypothesis but claim that their results leave them no alternative.

The present experiment is an attempt to yield a more precise quantification of the relationship between repeated presentation and probability of an item being learned. It is felt that such a quantification should provide a firmer base for argument than those measures used heretofore. To this end, the type of material used by Rock and other workers was employed in a rather different experimental design.

EXPERIMENTAL PROCEDURE

Rock (and most subsequent workers) replaced items which were not correctly associated on a given test. That is to say, the items presented in any list had either been correctly associated already or were being presented for the first time. The novel feature of the present experiment is that replacement was made of those items which were correctly associated on a given test. In this way none of the items presented in any list had yet been recalled correctly, and the items presented were either new or had previously been presented. This allows an investigation of the effect of repetition on the probability of an item being learned.

Material

A series of paired associate items was developed. The stimulus items were letters between D and Z inclusive, or double letters between DD and ZZ inclusive, and the response items were numbers between 10 and 58 inclusive. The letters I and II were excepted (since these are easily confused with the figures 1 and 11), and the numbers 11, 22, 33, 44 and 55 were excepted. It may be noted at this point that Rock's original experiment used all possible letters and double letters (excepting I and II), and numbers 1 to 50. The letters A, B and C were not employed in the present experiment, since it was considered that pairs involving these as stimulus items would be notably less difficult than those involving other letters (for instance, because of the English system of road classification). AA, BB and CC were correspondingly omitted. The numbers 1 to 9 were similarly omitted because these were felt to be easier than two-digit numbers.

A number was randomly allocated to each stimulus item to be used and these were stencilled on to 3 in. by 5 in. plain cards. On the other face of each card the relevant stimulus item only was stencilled. This was placed to the left-hand side of the card as it would have been if the response item had been present. On each trial a list comprised 12 such cards.

Method and apparatus

Items were presented to the subject by placing the list in a wooden rack such that all 12 cards were standing up behind one another. The cards leant back slightly, and only the front card could be read by the subject. This in turn was concealed by a plain white card until presentation of the list started. In this way, removal of one card exposed the next one to the subject.

After each presentation of a list of 12 cards there was a recall test, and the experiment was terminated after eight presentations and eight recall tests. The time intervals involved were as follows: presentation of each pair—3 sec.; interval between pairs—nil; before recall test—5 sec.; test presentation of stimulus item—5 sec.; after recall test—10 sec. All times were controlled by a metronome.

During presentations of a list each card was removed at the appropriate time and placed in another rack with the stimulus item only facing the front. This was done behind a screen, out of sight of the subject. The order of cards was varied between each presentation of a list and the ensuing recall test by placing each card in position in the second rack in a haphazard fashion. At the end of the list of 12 items the racks were exchanged

after the appropriate interval and the recall test started. A similar procedure was adopted for the recall tests, with the exception that, if the subject correctly recalled an item, the card was placed on one side and another taken from a (shuffled) pile and placed in a position which was haphazardly selected in the rack. In this way, 12 cards were presented on each trial, and the order of presentation was varied continually. A trial is defined as a single presentation of a list together with the recall test of items of that list.

During the presentations of a list the subject had to repeat aloud both items of the pair. During the recall tests, he had to repeat aloud the stimulus item and, if known, the associated number. No knowledge of results was given. The proceedings were recorded on tape and later transcribed. The cards used were randomly varied between subjects with the constraint that all were used approximately the same number of times for all subjects.

Subjects

23 subjects were used (13 male and 10 female). The majority of these were students of the Department, and the remainder research staff.

Instructions

Since it is felt that the instructions received by the subjects is an important variable in an experiment of this kind, the instructions given to them will be described in some detail. Identical instructions were read to each subject. These included details of the materials employed (see above), the fact that subjects had to repeat aloud what was presented, the distinction between presentations and recall tests and the number of trials involved in the experiment. They were asked at two different points "to learn the associations between the letters and the numbers." The possibility of new cards appearing was described in the following way: "After the first presentation and recall test, I will present a further series of 12 cards. These may or may not be the same cards. We will then have a recall test of these cards. And so on." No one was informed of the purpose of the experiment or about the manipulation of the cards until all subjects had completed the experiment.

Practice

Before the experiment proper subjects were given one presentation and one recall test using those letters and double letters which were not used later (i.e. A, AA, B, BB, C, CC, I, II), together with the numbers 90 to 97 randomly assigned to these stimulus items. This was primarily to accustom subjects to the time intervals involved and to the verbal responses required of them.

SCORING TECHNIQUES

The writer is of the opinion that the customary measure in paired-associate tasks of "items correct" is of only limited value. Such a score by itself gives only a part of the information required to describe a subject's performance in a learning experiment, and in isolation cannot yield a full understanding of the behaviour under observation. For instance, the "all-or-none" classification of a subject's responses as either "correct" or "incorrect," with no assessment of "partially correct" responses, must in itself determine to some extent the conclusion reached about whether learning is an all-or-none occurrence. Since the measuring technique employed can influence conclusions drawn from results in this way, it is quite possible that some generally accepted conclusions may themselves be the result of a limited categorization of experimental data. The results of this experiment have therefore been subjected to several types of scoring method, and it is felt that this allows of a fuller discussion of the behaviour of subjects.

The amplification of the customary scoring procedure suggested by Cook (1962), has been modified in several ways to yield the following measures:

"C-score," which is the number of items correctly recalled on a given test.

"S-score," which is the number of substitute responses emitted for the first time on a given recall test. A substitute response is defined as a response which is legitimate in that it appears in the current list but which is associated with an inappropriate stimulus item. S-score does not include replications of the same response term within one recall test, nor does it include the same substitute response emitted on recall tests after that test on which it was first emitted.

"P-score," which is the number of substitute responses emitted on a given test which have also been emitted on previous recall tests. P-score does not include replications of the same P item within one recall test.

"R-score," which is the number of replications of C, S and P items within one recall test. Whilst C-score, S-score and P-score yield a measure of the number of *different* responses emitted, inclusion of the R-score allows consideration of the *total number* of responses made which have been presented in the current list.

"I-score," which is the number of responses made on any recall test which appeared in former lists, but do not appear in the current list. I-score may be taken as a measure of interference from earlier lists. This score is of particular interest in an experiment of this kind, where lists are changing continually. For present purposes the I-score is taken to include replications.

"L-score," which is the total number of legitimate responses on any recall test, including replications of the same response. A legitimate response is defined as one which appears in the current list or which has appeared in previous lists. L-score is thus defined to be the sum of C-, S-, P-, R- and I-scores.

"X-score," which is the total number of extraneous responses emitted on a given recall test. Extraneous responses are those which do not fall into the category of legitimate responses. It must be pointed out here that many X-responses can be seen to have a certain "justification." Thus a response of, say, "36" could be interpreted as being a "half-correct" response "26" which appears in the list. Although such means may be employed to keep the X-score to a minimum, it is felt that to do so is to allow the factors of chance and subjectivity too much play. It is suggested that, unless very tight criteria are employed in this type of scoring procedure, the results obtained will have little reliability or validity.

"M-score," which is the total number of responses of all kinds made on a given test. M-score is thus the total of all preceding scores (i.e. L-score plus X-score). It is considered preferable to relate, say, L-score to M-score, rather than to the total response opportunities in the test (Cook's T-score), since subjects vary in their "guessing" tendencies.

RESULTS

Following these scoring procedures, subjects' mean performance over the eight recall tests is specified on Table I. It can be seen that the mean number of items learned (C-score) per subject was 19.65. There was considerable inter-subject variability, as evidenced by a S.D. of 8.89. The items were learned in a mean of 3.1 trials (S.D. = 0.62). Table I shows that the ratio of L-score to M-score remains consistently around 0.87. In other words, some 87 per cent. of responses made were legitimate or "meaningful" in some sense. Given this it can be seen that the customary C-score which constitutes rather less than 40 per cent. of all responses made, only provides a limited picture of a subject's performance. Subjects have clearly learned a considerable amount which will not show up in the C-score.

Table I exhibits the finding that mean M-score remains fairly constant over the eight recall tests with the exception of a low score on the first. The low M-score for test 1 is due to an I-score and a P-score which are necessarily zero and to a low C-score for that test. The latter finding is shown in Figure 1, where mean C-score and mean S-score are plotted against recall tests. A "typical" learning curve is of course not to be expected in this experiment, since the items to be learned were changing over the trials. The limited rise in C-score from tests 2 to 8 might, however, be attributed to increasing familiarity with the materials and procedure employed. The mean C-score on test 1 of only 1.13 will be considered below. The relative constancy of the mean S-score over the eight recall tests is attributed to increasing interference as the experiment progressed from items previously presented but replaced (i.e. I-score), and to the recurrence of substitute responses over successive recall tests (i.e. P-score). It will be recalled that S-score is a measure of only the first emission of substitute responses.

The experimental design allowed the repeated presentation of an item to be related directly to the probability of that item being learned. This relationship has been extracted in Table II. The probability measure used is of the "relative

frequency" type. For instance, 658 items were presented once, of which 84 were correctly associated. The probability of a correct response being made after one presentation ($p(C_1)$) is taken as $84/658$, which is 0.128. The $p(S)$ measure is on the same basis, and corresponds to the S-score referred to earlier. Thus $p(S_1)$ is

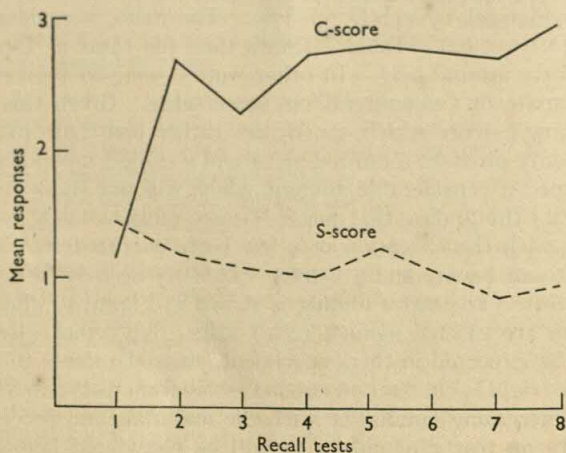
TABLE I
VARIOUS SCORING METHODS APPLIED TO EACH RECALL TEST

	Recall tests								Total	
	1	2	3	4	5	6	7	8	Mean	S.D.
Mean C-score	1.13	2.61	2.22	2.65	2.74	2.70	2.61	3.00	19.65	8.89
Mean S-score	1.48	1.13	1.04	1.00	1.26	1.04	0.83	0.96	8.74	3.73
Mean P-score	0.00	0.48	0.78	0.65	0.39	0.43	0.48	0.70	3.91	3.41
Mean R-score	0.69	0.87	0.83	0.66	0.87	0.57	0.78	0.86	6.13	6.50
Mean I-score	0.00	0.22	0.52	0.74	0.61	0.91	1.17	1.00	5.17	3.54
Mean L-score	3.30	5.31	5.39	5.70	5.87	5.65	5.87	6.52	43.61	15.61
Mean X-score	1.00	0.91	0.78	0.78	0.61	0.83	1.00	0.70	6.61	7.85
Mean M-score	4.30	6.22	6.17	6.48	6.48	6.48	6.87	7.22	50.22	16.80
Ratio L:M	0.77	0.85	0.87	0.88	0.91	0.87	0.85	0.90	0.87	—

C-score is the number of items learned. S-, P-, R-, I- and X-scores are various categories of what are commonly termed "errors."

For full explanation see text.

FIGURE 1



Mean number of correct responses (C-score) and mean number of new substitute responses (S-score) for each recall test.

the probability of a substitute response being emitted for the first time after two presentations of that particular response item. A clear distinction must be made between the terms "trial" and "presentation" as used here. "Trial 4" refers simply to the fourth trial in the experiment, in such a way that a trial consists of one presentation of the list followed by a recall test. "4th presentation," however, means

that an item has been presented four times. These may have taken place on trials 1 to 4, 2 to 5, etc.

In Table II it might be expected that the figure for the number of items presented n times, minus the C-score for n presentations, should equal the number of items presented $n + 1$ times. However, this is not the case since the items remaining unlearned after n presentations on the 8th trial are not presented $n + 1$ times.

Care must be taken not to attach too much reliability to the figures for $p(C)$ and $p(S)$ for larger numbers of presentations (say, 7 and 8), since the number of items receiving these many presentations was necessarily few. No matter how many trials are involved in an experiment of this kind, figures for the ultimate presentations must always be somewhat unreliable. Conditions in the present experiment did not favour really rapid learning in order that the reliability of these figures might be increased as far as possible.

TABLE II

THE RELATIONSHIPS BETWEEN C-SCORE AND S-SCORE AND NUMBER OF PRESENTATIONS OF ITEMS

	Successive presentations of a given item							
	1	2	3	4	5	6	7	8
Items presented	658	525	354	251	169	113	80	57
Total C-score	84	139	74	62	43	22	18	10
$p(C)$	0.128	0.265	0.209	0.247	0.254	0.195	0.225	0.176
Unlearned items presented ..	574	386	280	189	126	91	62	47
Total S-score	60	52	38	20	12	7	8	4
$p(S)$	0.105	0.135	0.136	0.106	0.095	0.077	0.129	0.085
Ratio $p(C):p(S)$	1.22	1.96	1.54	2.33	2.67	2.53	1.74	2.07

$p(C)$ is the probability of a correct response, and $p(S)$ is the probability of a new substitute response (excluding replications of the same response). The purpose of computing the ratio $p(C):p(S)$ is considered in the Discussion.

The significance of the ratio $p(C):p(S)$ shown in Table II will be discussed below. Table II has been represented graphically in Figure 2, when it can be seen that $p(C)$ remains fairly constant after the first presentation. If anything, $p(C)$ declines with increasing number of presentations after the first one. It is clear that $p(C_1)$ is very much lower than $p(C_{2+})$. It may be argued, however, that the low $p(C_1)$ is an artefact stemming from the low C-score on test 1. The justification for this is that (23×12) items received their first presentation on trial 1. Since C-score on test 1 is much lower than on later tests, it is possible that this has affected $p(C_1)$ which is a measure of learning after one presentation of an item irrespective of the trial on which that item first was presented. To check this possibility a new $p(C_1)$ was computed excluding those items first presented on trial 1. This probability is 0.152 and is plotted as point C in Figure 2. It can be seen that this is not markedly different from $p(C_1)$ so that the results remain essentially the same. The low C-score on test 1 is, therefore, not due to this being the first trial of a task which might later be improved by practice, but, rather to the fact that all items had received only one presentation.

The significance of the difference between $p(C_1)$ and $p(C_{2+})$ and between point C

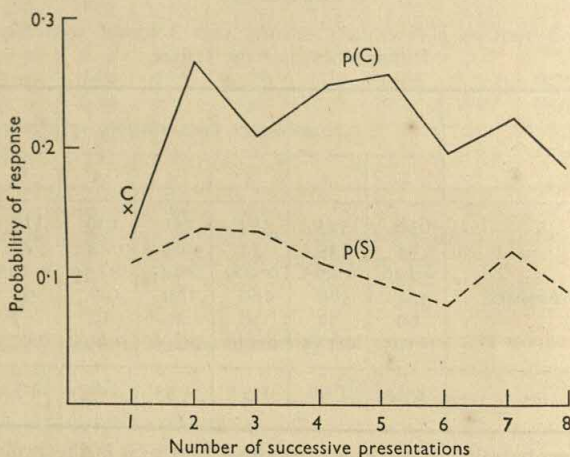
in Figure 2 and $p(C_{2+})$ was investigated by means of Wilcoxon matched-pairs signed-ranks tests. In both cases it was found that $p < 0.001$.

DISCUSSION

The established relationship between repeated presentation of an item and the probability of that item being learned (the $p(C)$ curve in Figure 2) cannot be predicted on either an incremental or a single-trial hypothesis. A purely incremental account of paired-associate learning demands that $p(C)$ increases in some regular manner with repeated presentations of an item, and a single-trial account requires that $p(C_1) = p(C_2) = p(C_3)$, etc. Neither of these relationships is found to obtain.

The striking points about the $p(C)$ curve in Figure 2 are the marked discontinuity between $p(C_1)$ and $p(C_{2+})$ and the decline from $p(C_2)$ to $p(C_8)$. As noted above,

FIGURE 2



The probability of a correct response ($p(C)$) and of a new substitute response ($p(S)$) after each number of presentations.

$p(C_{2+})$ is significantly greater than $p(C_1)$, indicating that learning is improved by more than one presentation of items. The decline from $p(C_2)$ to $p(C_8)$ apparently indicates that further presentations after the second impede learning. This decline can, however, be shown to be due to the fact that not all subjects require eight presentations to learn these items; not all subjects whose performance is represented in the calculation of earlier $p(C)$ values are involved in the data analysed to calculate later $p(C)$ values. To study the trend from $p(C_2)$ to $p(C_8)$ it is therefore necessary to investigate the effect of repetition upon $p(C_{2+})$ for individual subjects. This analysis can be carried out by computing the rank correlation between $p(C_2)$, $p(C_3)$, ..., $p(C_n)$, and number of presentations for individual subjects. A positive correlation of this kind indicates that the subject in question has benefited from repeated presentation of items. The performance of the group can be represented by the mean of the sample of individual correlations. The mean correlation (ρ) under these conditions was found to be $+0.01$, which does not depart significantly from zero. It may therefore be concluded that the decline from $p(C_2)$ to $p(C_8)$ exhibited in Figure 2 is an artefact, but that nevertheless the group has derived no benefit from additional repetitions after the first one. Items learned after one presentation (i.e. those

involved in $p(C_1)$ are by definition examples of single-trial learning. We clearly cannot argue that single-trial learning *never* occurs. Those items learned after 2 to 8 presentations, i.e. those involved in $p(C_2)$ to $p(C_8)$, must be examples of one of the following:—

- (a) incremental learning,
- (b) single-trial learning,
- (c) a combination of (a) and (b).

That they are not examples of (a) is clear from the results, since this demands that $p(C)$ rises with increasing numbers of presentations. That they are not examples of (b) must be inferred from the discontinuity between $p(C_1)$ and $p(C_{2+})$ since, if they were so, $p(C_1)$ should equal $p(C_{2+})$. If, however, we hypothesize that some single-trial learning does take place irrespective of the number of presentations, but that a second factor comes into play on the second and later presentations (i.e. an incremental factor) we can provide a tentative explanation of the data. Such a "dual factor" hypothesis requires that the single trial and the incremental factors make an additive contribution to learning a list of items. Items learned after one presentation (i.e. those yielding $p(C_1)$), naturally do not involve the incremental factor.

The notion that "single-trial learning does take place irrespective of the number of presentations" clearly requires some elucidation. Suppose that on trial 1 some "trace" is formed within the learner between each of the twelve stimulus items and its respective response item. It is logically possible for this to happen and yet for the learner to obtain a low C-score on the ensuing recall test, since forgetting and interference will take place. We might, however, argue that those items unlearned and therefore recurring on trial 2 in this experimental design do have some advantage over new items, and will therefore show a higher $p(C)$ than new items on that trial. Thus far we are in accord with the results, but we cannot invoke the same explanation for trial 3. For items presented on trials 1, 2 and 3 would be assumed to have yielded three "traces," whilst those presented only on trials 2 and 3 would be assumed to have yielded only two "traces." In this case $p(C_3)$ should be higher than $p(C_2)$. The same argument might be made for all probabilities up to $p(C_8)$. It receives no support from the results, as described by the $p(C)$ curve in Figure 2.

We might, therefore, argue that subjects in some way "pay more attention" to only certain items in the list. This obviously does occur on some occasions, as when subjects regularly learn on a single presentation the pair including their initial as a stimulus item. Despite the fact that subjects in the present experiment had to repeat aloud all items in the list, discussion with them after the experiment revealed that many of them adopted the strategy of attempting to learn only certain items. Let us term this strategy one of "selecting" certain items. The factors of interference and forgetting render it possible that only some of these selected items will be recalled correctly. But if only a proportion of items are selected for learning on the next trial also, it is not necessary that these will include those selected (but not recalled) on the previous trial. In this case no incremental factor would be observed. Suppose however, that there is a fairly constant overlap between items selected on successive trials so that some limited benefit is derived from repeated presentation of a series of items. This is consistent with the finding that items presented for learning more than once have a very significant advantage over those presented only once, whereas this advantage does not increase between 2 and 8 presentations.

One hypothesis which provides a possible explanation of the results is, therefore, that the two factors of single-trial learning and incremental learning both contribute

to the learning of paired associate lists, and that items are in some sense selected for learning from the list presented. Subjects' selection of items will be determined by a number of factors. Amongst these we must include the "meaningfulness" of certain stimulus or response items or of the linking of two items of a pair, intralist and interlist similarity, time schedules, serial order effects, instructions given and a subject's conception of his task. Incremental learning may in this way be regarded as resulting from the failure to learn certain items on a single presentation only in so far as these items are selected for learning on a number of presentations. As a hypothesis this generates several predictions, confirmation of which must precede any confident assertion of its validity.

The dual factor hypothesis can predict those very results obtained by Rock (1957), and generally interpreted as evidence for single-trial learning. Although Rock's experimental group was presented with some new items on each trial, it is conceivable that his control group was presented with some *effectively* new items on each trial. This would be due to the selection of certain items from a list, so that non-selected items on trial 1 are effectively new on trial 2. If we suppose that all selected items were learned by both groups after a single selection, no disadvantage would be suffered by the experimental group for whom all unlearned (i.e. unselected) items were replaced on the next trial. On the other hand, when conditions are such that only a small proportion of the items selected are likely to be learned, the experimental group is found to be significantly worse than the control group (Lockhead, 1961). In other words, we can argue that Rock's results exhibit the effects of "single selection" learning of all selected items. On the present hypothesis it is argued that whilst Rock's experimental conditions favoured single selection learning, Lockhead's conditions (deliberately made more difficult) were such to necessitate incremental learning as well. Clearly, Rock's hypothesis of single trial learning provides an economic explanation of his own results, but, since the single trial hypothesis proves to be too economic to explain the present data and those of Lockhead, we must seek a more complex explanation of all sets of results. The present hypothesis provides one such explanation.

It is of interest to consider the results of the present experiment in relation to the notion that paired associate learning involves two phases—the learning of responses and the learning of associations (Underwood, Runquist and Schulz, 1959; Horowitz, 1961). Similar conceptions involving a greater number of stages have been suggested (Cook and Kendler, 1956; Murdock, 1959; McGuire, 1961), but these agree in acknowledging the existence of response-learning and associative phases. The simpler schema will be used for expository purposes here, without attempting to judge the merits of the various conceptions. The following discussion in no way assumes that response-learning always occurs on an earlier presentation than association-learning. Whilst this may sometimes be the case, it is obviously not always so.

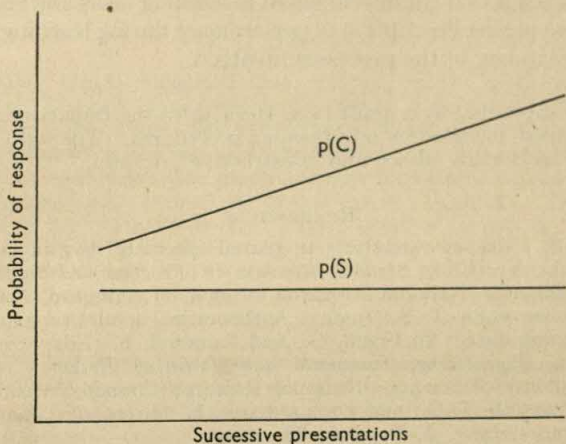
Consider the objects of the measures here termed S-score and C-score. S-score clearly only measures the subject's learning of responses, since replications of a response within one recall test and over successive tests are omitted from this measure. S-score is thus a measure of the response-learning phase. Now, since C-score is a measure of correct responses, we can say that this is a measure of both the response-learning and the associative phases. Therefore, since S-score measures only the response-learning phase and C-score measures both the response-learning phase and the associative phase, to study the associative phase alone we must compare C and S in some manner. Such a comparison proves most instructive when it is based on the effect of repeated presentations of items to be learned.

Figure 2 exhibits the relationship of these two measures that has been established.

It has been argued that the shape of the $p(C)$ curve is such to suggest a dual factor hypothesis of paired associate learning. On the other hand, the relationship between $p(S)$, which is a measure of the response-learning phase, and repeated presentation is consistent with a single trial hypothesis. A substitute response has no greater probability of being learned after, say, eight presentations than it has after a single presentation. This hypothesis, that response learning is a single trial occurrence, is, of course, quite independent of the dual factor hypothesis which purports to explain the factors involved in paired associate learning as a whole in the present conditions.

Suppose for the moment, however, that Figure 3 exhibited the established relationship between $p(C)$ and $p(S)$. The $p(S)$ curve, being independent of number of presentations, would have to be interpreted as showing that the response-learning phase was single-trial occurrence. On the other hand, since $p(C)$ increases regularly

FIGURE 3



Hypothetical relationship between $p(C)$ and $p(S)$ curves. For discussion see text.

with repetition, we would have to argue that the response learning phase together with the associative phase exemplifies incremental learning. But since response learning is found to be a single trial occurrence (in these hypothetical results), then the associative phase would have to be explained on an incremental hypothesis. To state this more simply, if the ratio $p(C):p(S)$ increases regularly with an increase in the number of presentations, then the associative phase is a case of incremental learning.

The ratio $p(C):p(S)$ over increasing numbers of presentations is thus an indication of the factors involved in the associative phase of paired associate learning. To take another example, if the ratio $p(C):p(S)$ remains constant, then the associative phase would be seen to involve only a single trial factor. The ratio actually established in the present experiment is shown in Table II. This can be seen to be fairly constant between two and eight presentations, but with a very much lower ratio for a single presentation. In other words the relationship between this ratio and number of presentations is of the same kind as that between $p(C)$ and number of presentations. The hypothesis has previously been put forward that the $p(C)$ curve in Figure 2 may be explained in terms of the additive contribution of incremental and single trial factors. We may similarly hypothesize that both factors are involved in the

associative phase of paired associate learning, as exemplified by the ratio $p(C):p(S)$. A combination of this hypothesis and the hypothesis propounded earlier about the response learning phase yields the following possibility: whereas the response learning phase of paired associate learning in these conditions involves only single trial learning, the associative phase in these conditions involves both types of learning.

It is not suggested that this hypothesis is in any way fully confirmed by the present findings. It would, however, seem to be supported by the present results, and it is sufficiently precise to generate testable predictions. It is important to check the generality, or at least predicted variability, of these results over different kinds of material, different compositions of lists, different time schedules, etc. Methods are currently being developed of manipulating only one of the two phases discussed above (e.g. Horowitz, 1961). If the present findings prove to be reliable, such manipulations can possibly be employed to test predictions from the hypotheses tentatively put forward here. Whilst the task used in this experiment was, of course, an unusual one, it is felt that the development of learning tasks and scoring procedures which allow a more precise description of performance during learning is a prerequisite for deeper understanding of the processes involved.

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SHORTER ARTICLES AND NOTES

SHAPE DISCRIMINATION IN TROPICAL FISH

BY

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Two experiments are reported using Egyptian mouthbreeders as subjects in which circle/square discriminations were attempted. In Experiment I, using large stimuli in a maze type situation, no learning was shown by any of the six subjects. In Experiment II, using a conditioned operant technique of nose banging with smaller stimuli, some learning was shown by only one of the five fish. Rapid learning of other discriminations using the square or circle as negative stimuli was subsequently shown.

INTRODUCTION

Work by Lashley (1938) suggested that rats are unable to discriminate between circles and squares. Deutsch (1955) produced a theory of shape perception which would predict this experimental finding. Dodwell (1957) reported experiments in which groups of two rats were trained on circle/square discriminations, on a modified Lashley type jumping stand. The square positive group reached the criterion quickly, although the circle positive group had not attained the criterion after 150 trials. However, Dodwell ascribes the superior performance of the "square positive" group to their pretraining experience on vertical and horizontal striations. Dodwell (1960) with rats (pretrained to dots on a white card rather than to striations) found that the group (three animals) being trained on small squares and circles of the same area, with square positive were not jumping to the square at a better than chance level after 400 trials. However, by this trial the performance of a control group, jumping to a "positive" triangle with the circle negative, had reached the criterion at the 0.05 level (Chi square test). Dodwell does report that the experimental group were performing at a better than chance level 0.05 level chi square test) during trials 301-350. Sutherland (1957) reports that octopi given circles and squares as stimuli with a response to the negative stimulus followed by electric shock responded to the positive stimulus on 69 per cent. of the presentations.

The series of experiments to be reported here set out to investigate the ability to discriminate circles from squares in the Egyptian mouthbreeder (*Tilapia Macrocephalus*) over a variety of experimental conditions. An experiment carried out by the writers on sticklebacks (*Gasterosteus Aculeatus*) had earlier suggested that the circle/square discrimination would prove difficult for fish as compared with circle/triangle discrimination.

EXPERIMENT I

Apparatus

This experiment was designed to study the discriminatory behaviour of the Egyptian mouthbreeder in a maze situation. The design and dimensions of the maze are shown in Figure 1. It was made of white perspex, but the choice and approach areas were painted black. The vertically sliding door between the entrance and choice areas ensured a central approach to the choice wall in which the stimuli were cut. These were two equally sized circles situated on each side of a square (all of the same area) with the lowest portion of each figure one half inch from the floor of the maze. The side length of the square was $\frac{7}{8}$ in. long. This included the black outline of the stimulus which emphasized the shape of the aperture. The wall could be moved sideways across the maze to randomize the position of the stimuli. The subjects were required to swim through the stimuli into the reward area. A small pellet of food was dropped from an electrically operated dispenser into the appropriate side of this area after a correct choice had been made. Retracing from the reward area was prevented by an additional sliding door. The maze was suspended in the water of the home tank of each fish (which were kept individually) so that the depth of water in the maze was always 2 in. The various

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moving parts were operated manually using attached metal rods (not shown in Figure 1) and the movements of the fish were observed by means of a mirror fixed at an angle above the maze.

Subjects

The subjects were six Egyptian mouthbreeders obtained from a local supplier and at the time of commencing the experiment, they were approximately 3 months old.

Procedure

The subjects were fed close to the maze for one week before the actual experimental schedules began. At the end of this period, they approached the maze when it was placed into the water and swam into the entrance box. The door between this and the

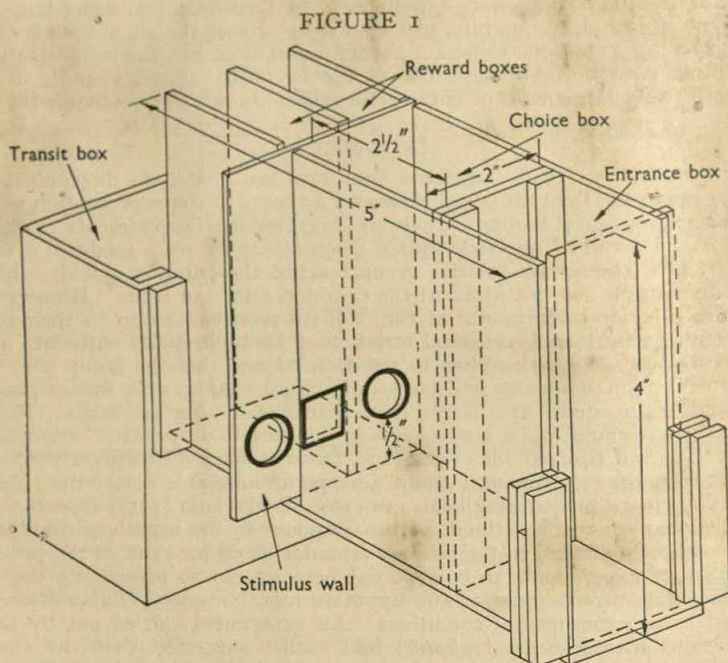


Diagram of maze as used in Experiment 1.

choice box remained closed. To begin a trial the door of the choice box was raised. The fish were deprived of food for 22 hr. preceding the first trial of each day and this schedule was maintained throughout the experiments. For three fish the square was positive and for the others the circle. Immediately a correct response was made, one pellet of food was dropped into the appropriate reward box. No reward was given after incorrect choices. When the fish moved into the transfer box, this was detached from the end of the maze and attached to the entrance area. Ten trials per day were given and half an hour after the final trial in the daily series, the number of food pellets received by the fish was made up to ten, depending on the number of correct choices made. The inter-trial interval between the arrival of the fish in the transfer box and its subsequent release into the choice box was approximately 10 sec. If, in any one trial the fish had not responded after 15 min. the daily series was discontinued. The experimental period covered 25 consecutive days giving a potential total of 250 trials.

Results

The numbers of responses made, the total number of correct trials, and the percentage correct responses of the total number of trials attempted by each fish, is given below.

Discussion

The results of this experiment and a detailed inspection of the day-to-day records indicate that no learning had apparently taken place over these trials. Rather interestingly, three of the fish declined to respond after the first few days' trials and generally showed signs of behavioural disorder (e.g. abnormally rapid movement about the tank on release, lack of movement in the maze). This could be interpreted in terms of the difficulty of the discrimination (e.g. Pavlov, 1927), either in terms of the shape, size or colour of the relevant stimuli. Previous exploratory work has indicated that the colour of the stimuli is not a relevant variable. It could be argued that the visual field of the fish would not be large enough at the given distances from the stimuli (i.e. maximally 2½ in.) and that partial sampling of the stimulus shapes might lead to inadequate responses. As it was not possible to produce alternative shapes of the same area through which the fish were able to swim, a subsequent test of the learning ability of these fish in the maze was not made. Data obtained from an earlier experiment with sticklebacks, which having failed to learn a circle/square discrimination subsequently learnt a square/triangle discrimination, with slightly larger stimuli than they originally responded to, suggest that

TABLE I

Fish	Positive stimulus	No. of responses made	No. of correct responses made	Percentage of total
1	Circle	77	36	46.7
2	Square	66	31	48.4
3	Circle	67	31	47.3
4	Square	228	109	47.8
5	Square	250	131	52.4
6	Circle	231	113	48.9

These are clearly not significantly different from chance.

this is not a crucial variable, particularly as the maximum distance in a return T maze which these fish could be from the stimuli was only 1 in. A more direct test of this problem is reported in Experiment II. Generally, then, these results suggest that the finding that some rats are unable to learn a circle/square discrimination, may be extended to cover one species of tropical fish (*Tilapia Macrocephalus*) in a maze situation using large stimuli.

EXPERIMENT II

Introduction

The work of Bitterman and his co-workers (e.g. Wodinsky and Bitterman, 1959, etc.) demonstrate considerable success with a conditioned operant technique in experiments with fish. A similar method was used here in order to introduce smaller stimuli than those in the maze experiment, to which objections might be made in terms of the potential visual field of the fish. It was also hoped that this technique would enable us to prolong the period in which the animal would respond and explore other dimensions potentially relevant to the response (e.g. colour of stimuli, spatial separation of stimuli, latency as a measure of response).

Apparatus

A metal gantry was constructed on which were two sliding metal arms. The gantry could be placed in front of the narrow end of the home tank so that the arms projected over it. Fixed into gramophone stylus cartridges attached to the end of the arms were light metal rods to which the stimuli were glued. The stimuli were of dark brown bakelite sheet $\frac{1}{16}$ in. thick, the square being of side $\frac{3}{16}$ in. and the circle constructed to be of equal area. Also projecting over the edge of the tank between the two stimulus arms was a circular clear perspex hand operated food dispenser. When the gantry was in the operating position the stimuli were $\frac{1}{2}$ in. below the surface of the water, $1\frac{1}{2}$ in. apart and $\frac{1}{2}$ in. from the glass wall of the tank. When the fish made a response the gantry was pivoted backwards, taking the stimuli out of the water to enable the positions to be changed.

If the response was correct, a pellet of food was immediately dropped on to the surface of the water, centrally between the two stimuli. Recording of response was by means of an Ediswan electroencephalograph recording device connected to the gramophone cartridges. Microswitches which made a short circuit across the recording device when the stimuli were removed from the water and restored the current when they were replaced enabled a measure of latency to be obtained.

Procedure

The operant response of nose banging was established by presenting the subject with a single neutral stimulus (a triangle) placed centrally in the tank, for five rewarded trials per day for 35 days. At this point one fish died and it was not possible to replace it. In both pretraining and the experimental sessions the fish were deprived of food for 22 hr. preceding the first trial of each day.

FIGURE 2

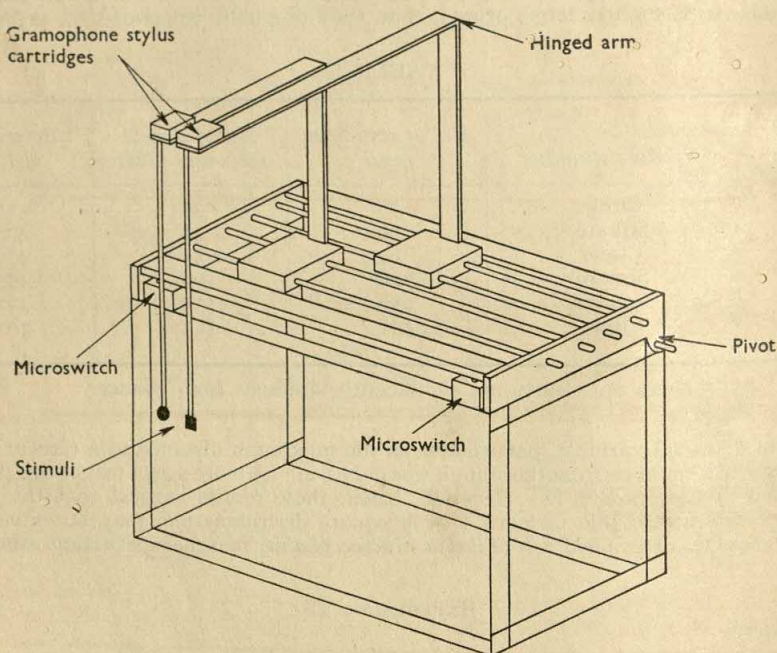


Diagram of bar-pressing apparatus as used in Experiment 2.

The experimental period was divided into three parts:

- I. Five trials per day were given for 10 days using the square and the circle.
- II. Ten trials per day for 19 days using the square and the circle.
- III. Ten trials per day for 7 days (except Fish 2, see Table IV) using four combinations of stimuli of equal area (see Table II for details).

All the stimuli were presented in a randomized physical position according to Gellerman (1933) orders.

In parts I, II and III the intertrial interval varied slightly, but approximated to 10 sec. The variability depended on the behaviour of the fish after the operant response had been made, for the stimuli were not replaced in the water until the position of the fish was judged to be equidistant between the two stimuli. The distance from the stimuli varied, but as the fish normally remained within the area where food had been delivered was usually between 1 in. to 2 in. from the position of the stimuli. As the sequence developed into a rapid approach, response to a stimulus, and then a short withdrawal, it was possible to take both stimuli out of the water without disturbing the fish.

TABLE II

Fish	Parts I and II	Part III	
	Positive	Positive	Negative
1	Square	Triangle	Circle
2	Circle	Rectangle	Circle
3	Circle	Triangle	Square
4	Square	Rectangle	Circle
5	Circle	Rectangle	Square

Subjects

These were the same fish that had been used in experiment I, and at the start of training were approximately 3 months old.

Results and statistical analysis

- (1) Total scores for each fish are given below in Table III.

TABLE III

Fish	No. of responses made	No. of correct responses
1	240	124
2	194	108
3	240	124
4	240	143*
5	240	115

(* Significant (χ^2 test) 0.01 level.)

- (2) Further training on different pairs of stimuli after 240 trials on the square/circle discrimination.

TABLE IV

NUMBER OF CORRECT CHOICES OUT OF TEN (EXCEPT FOR FISH 2 WHERE TOTAL NUMBER OF TRIALS AND CORRECT RESPONSES IS STATED IN TABLE)

Days								(Ten trials per day)	
Fish	1	2	3	4	5	6	7	+S	-S
1	7	6	8	9	10	9	10	Triangle	Circle
2	3/5	No. R.	No. R.	No. R.	6/7	8/9	10	Rectangle	Circle
3	5	4	7	8	8	10	10	Triangle	Square
4	5	4	8	7	9	10	9	Rectangle	Circle
5	7	8	10	8	9	9	9	Rectangle	Square

Over the last 4 days the performance of each fish (except fish 2) is significantly better than chance beyond the 0.001 level (χ^2). For fish 2 this applies over the last 3 days.

Discussion

As only one fish from this group was performing at a better than chance level after 240 trials the data do not lend strong support to the view that a circle/square discrimination is possible for this type of fish and confirm the findings of Experiment I. The fish

which did respond above chance level over the whole series was extremely variable in the number of correct choices made, ranging from 3 to 9 over the last 8 days. This fish had been pretrained on a triangle and it could be argued that this effect might be due to transfer effects from one straight sided figure to another with straight sides.

The subsequent rapid attainment of efficient performance in the final part of this experiment (see Table III) indicated that the failure to discriminate must be due to the characteristics of the stimuli and so lend some support to the view that lower animals are not able to make this discrimination. These results tend to support Deutsch's theory of shape perception.

One incidental observation may support the view that restriction of movement is an important factor in the production of the behavioural disorder in a difficult discrimination situation. In the first experiment, with a limited size choice area, five of the six fish stopped responding at some time or other, whereas in the greater area of the home tank with presumably a similar task only one fish out of five refused to respond in each of the 240 trials, and this fish when given an easier discrimination in the final section of the experiment rapidly resumed responding and reached a high level of success in fewer trials than the other fish (see Table IV).

Additionally, we would like to mention several findings incidental to the one in which we were particularly interested. Latencies gave no differential measure in any part of these experiments. Stimuli of at least two colours elicit responses even without the shape being emphasized by an outline. Controlled operant responding appears likely to produce more useful data with fewer disturbing artefacts than the maze situation which we used (for these fish at least). Finally, tropical fish (three different types have been used to date as experimental subjects by the writers) appear far more suitable for this type of investigation than cold water fish being generally voracious and very active.

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SOME REMARKS ON DEUTSCH AND CLARKSON'S "A TEST OF THE NEC-BEHAVIOURISTIC THEORY OF EXTINCTION"

BY

JAMES R. ISON

From The University of Rochester

An experiment by Deutsch and Clarkson (1959) was presented as a refutation of an extension of the Hullian theory of extinction offered by Moltz (1957). Two groups of rats were given a series of free reinforced trials in a square maze in which either a left or a right turn at the choice point led, via two turns in the opposite direction, to a single goal box. On a first test day Group A was prevented from reaching the goal by blocking both routes whereas Group B was permitted to reach the goal which had the usual food reinforcement removed. A second series of free reinforced trials was followed by a second test day on which the conditions for the groups were reversed. The important observation was the frequency of alternation on the second trial of each test day. It was supposed from Hullian theory that no difference in alternation between the two conditions was expected but under a model proposed by Deutsch (1953) it was predicted that blocking would result in greater alternation. Under the latter position entrance to the empty goal would not increase the probability of the alternate response because the goal was the terminal unit to both responses and thus "both stimulus series had been interrupted."

However, the basic question for Hullian theory, and this applies to Moltz's position also, is which condition would provide the greater response decrement. It seems apparent that blocking would produce the greater decrement because there would be no secondary reinforcement for the response. This was recognized by the authors but they do not believe it to be important: "one should not expect apparently no effect in one case and a large effect in the other" (Deutsch and Clarkson, 1959, p. 149). However, that this is a crucial variable was demonstrated in an experiment by Klugh (1961). Five groups of rats received 20 reinforced trials in a runway followed by 14 extinction trials. In extinction one group was blocked from the goal and the other four were detained in the goal for 3, 30, 240 or 720 sec. The blocked group exhibited less response strength than any of the other four which, with a slight inversion between 3 and 30 sec., ranked in decreasing resistance to extinction. These data support the Hullian position—although within the context of Deutsch's model they present a problem because all groups have "an interruption of the stimulus series."

It should be noted that this prediction follows for either the one or the more usual two goal box apparatus. A further effect of entrance into the single goal is to extinguish in part the conditioned fractional goal response (rg) to the extramaze cues of the goal box ("place" cues). These are visible from the choice point and such extinction would lower the strength of any approach to them, either a right or a left turn. Blocking on one side of the maze would extinguish rg to the extramaze cues on that side and thus produce a differential effect at the choice point. (The importance of extramaze cues to the latent extinction phenomenon has been demonstrated by Denny and Ratner, 1959.) In addition, if the subject were allowed to retrace partially from the blocked area avoidance responses would be conditioned to both intramaze and extramaze cues on that side of the maze and further accentuate the effect.

Thus both theories appear to predict the same behaviour, that blocking should produce greater alternation than entrance to the unbaited goal box.

One further comment remains, that the χ^2 statistic was employed in a common but incorrect way (Lewis and Burke, 1949). Deutsch and Clarkson chose to analyse the data by groups, taking Group A for a first analysis, Group B for a second and combining Groups A and B for a third. Thus two entries in each table were contributed by each subject which clearly violates a fundamental requirement for χ^2 that the frequencies be independent. An appropriate design for analysis is by test series with alternation vs. no alternation as one dimension and blocking vs. entry as the other. This would yield two tables, Test 1 and Test 2, in each of which all entries are independent although one table is

not independent of the other. Although these tables satisfy the independence requirement for χ^2 the expected cell frequencies (based on Deutsch and Clarkson's observations) are too low for the tabled values of χ^2 to give a good approximation to the actual probability values (as, indeed, were the expected values in the tables erroneously used). Fisher's exact test (Walker and Lev, 1953, p. 204) is appropriate and yields for Test 1 $f = 0.026$ and for Test 2 $f = 0.091$. These f values may be halved because of the directional prediction afforded by the theories. Thus each test permits rejection of the null hypothesis at an acceptable level.

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BOOK REVIEWS

Social Psychology Through Experiment. Edited by George Humphrey and Michael Argyle. London: Methuen. 1961. Pp. 208. 21s.

The excellent plan of this book was to get together a number of research workers who have been actively concerned in developing experiments claimed to have a predominantly social interest and application, to invite them to describe briefly work in their chosen fields, and, in more detail, some specially selected experiment, or experiments. The hope was that readers would then repeat, and develop, these selected experiments, or at least some of them, and thus learn, in the best, and perhaps in the only possible way what social psychology is aiming to do and how far it can achieve its aims through the use of controlled methods. Obviously, therefore, judgement as to the value of the book must depend upon whether the hopes of its editors and contributors are realized, and with what ultimate result; and such judgement must wait for a while.

It can be said at once, however, that this book is a brave attempt to do something new in aim, though it does not pretend to introduce anything new in fact. The topics dealt with are: Social Perception (Henri Tajfel), Motivation and Conflict (Peter Robinson and Michael Argyle), Communication (A. N. Oppenheim), Small Social Groups (Thelma Veness), Interviews (D. R. Price-Williams), Social Class (Gustav Jahoda), and Small-Scale Social Surveys (J. F. Morris). Each chapter is followed by a selected bibliography. It is of some interest that there are rather more than twice as many references to American work than there are to British, and that the only topic for which British authorities are in the majority is Small-Scale Social Surveys. This topic has produced one of the best essays in the book.

Very likely it will be said that the issues dealt with are rather trivial; that no attempt whatever is made to consider the urgent problems of contemporary social life, economic perhaps, or political, which now trouble men's minds everywhere; that it does not even become clear what genuinely makes any of the alleged factual material social. Most of such criticism would be out of place, since it is made clear by the editors that all that is going to be discussed are existing experimental approaches. It would perhaps have been relevant, however, and would certainly have given the book an added interest, if some effort had been made to consider how, if at all, existing experiments might be further developed to deal with at least some of the wider issues.

More troublesome, as the volume now stands, is the varied and frequent use made of hypothesis, assumption, guesses, generalization and even codifications—terms which, though they are nowhere clearly defined, tend to be treated as more or less interchangeable. Maybe the frequent and uncritical appearance of these words and their corresponding notions is in fact due to the predominant American influence. The underlying assumptions seem, in a number of cases, to be expressions of what is treated as known about social factors or relations before an experiment begins, or, at least, as extra to the experiment itself. Then, if the experiment is arranged to reveal them in action, this comes to be treated as in some sense demonstrating, or proving them. A slightly sceptical reader will probably ask how much social psychology has got to be known before social psychological experiment can be conducted. This, perhaps, points to a methodological difficulty which may have wide applications beyond social psychology itself: it might well have been more carefully considered by one of the editors.

A word should be added about Professor Humphrey's *Introduction for Humanists*. This is a brief, pointed, lively and attractive justification of the use of experiment in the attempted solution of human problems. If a psychologist—for whom it has not been written—starts by reading this, he will enjoy it, but may think it, at the present stage of things, unnecessary. By the time he has finished the book, however, he will see that it is very much to the point.

F. C. BARTLETT.

Society: Problems and Methods of Study. Edited by A. T. Welford, Michael Argyle, D. V. Glass, J. N. Morris. London: Routledge and Kegan Paul Ltd. 1962. Pp. vi + 586. 50s.

Just before the Second World War a work entitled *The Study of Society* was published. It was edited by F. C. Bartlett, M. Ginsberg, E. J. Lindgren and R. H. Thouless, and contained 19 chapters divided into four parts dealing with social psychology, social applications of psychological tests, social anthropology, and sociology. This book was

"addressed mainly to those who are engaged upon, or who wish to engage upon, social research, and who require to know what methods are already available, upon what established conclusions they may reasonably build, and what are some of the outstanding problems which might immediately repay further study."

Now, over 20 years later, has appeared a comparable work with a similar aim, being intended "for those setting out upon a systematic study of social processes, and also for those whose main interests lie elsewhere but who wish to orient themselves in particular parts of the field." We therefore have something of a yardstick, both to evaluate the book under review and the development of studies of society.

There has, of course, been enormous progress, but is it quite as great as today's editors believe? They observe in their introduction that "a few years ago such a book would have been fairly brief and yet inclusive." But *The Study of Society* is only some 80 pages shorter than the later work. This, of course, is a relatively insignificant measure, especially since the editors have omitted anthropology and other studies less relevant to Great Britain. There have clearly been great developments of technique, especially in statistical analysis. A number of new spheres of enquiry have opened up as reasonable fields for scientific study, operational research, for example, hospital sociology and the study of small groups. The methods of experimental psychology have been successfully applied in new fields, such as geriatrics. Of great importance has been the more close relation of psycho-therapeutics and social process. In making this comparison of then and now, it is particularly interesting to contrast the contributions of the two writers, Professors Rodger and Vernon, who contributed chapters on very similar themes to both volumes. In 1939 Professor Vernon concluded that while "correlational studies and empirical investigations of the reliability and validity of the scales . . . are of undoubted value . . . testing and statistical techniques have outstripped our knowledge of their proper application and interpretation." Twenty-three years later, after innumerable, "correlational studies and empirical investigations" have been carried out, he writes in a way which would suggest that his knowledge has caught up with his technique. In the first book Professor Rodger writes with knowledge and practical good sense about the work of the vocational adviser. In the second (with Mr. Cavanagh) he broadens his approach greatly and makes use of a great deal of psychological literature. But neither Professor Vernon nor Professor Rodger seem to indicate anything approaching a radically new type of understanding. This is not, of course, a criticism of two distinguished psychologists, but a somewhat surprised comment, which could be extended to the two books almost *in toto*. There has been a vast increase in competence, a broadening of scope, but surprisingly little that is really new in kind—at least, as reported here. Despite the tremendous increase in investment in the social sciences there is no hard evidence of a breakthrough in understanding. It is interesting to note, for example, how many authors in *Society* refer to the key concepts of Durkheim, Weber and, among the modern giants, Sir Cyril Burt. It is particularly significant that in one field in which distinct progress does seem to have been made—the understanding of the relationship of social milieu to mental illness—the author (Dr. Russell Davis) concludes as follows: "If honour is paid to Freud, it should also be paid to his contemporaries: Trotter, who pointed out again the protective function of social groups, and, in particular, Durkheim, who attributed certain types of suicide to failure of integration of the individual into society."

But whether or not we consider that studies of society are developing adequately, this is an admirable book which helps us to see clearly just how well they are doing in Britain. It is divided into two parts, the first entitled "Approaches and Methods of Study," the second, "Problems and Application." Part II is subdivided into sections on Industry, Social Pathology, Population Studies and Problems, and Aspects of Sociology. In all there are 31 chapters, each a model of clarity and a mine of information on such varied topics as statistical surveys, road accidents, the inheritance of personality disorders and national intelligence.

If any of us are here in 20 years' time another, equally good, stocktaking would be of great value.

ADAM CURLE.

Sleeping and Waking. By Ian Oswald. Amsterdam: Elsevier Publishing Co. 1962. Pp. ix + 232. D.fl. 22.50. 45s.

This book, as the title suggests, is a natural successor to Kleitman's *Sleep and Wakefulness*, published in 1939, and concentrates on the important advances which have taken place since that time. It starts with a general account, of the organization of brain-stem mechanisms, and of the changes in the E.E.G., G.S.R., and other psychological

variables with sleep and arousal. Those who have followed Oswald's papers over the past few years will have a fairly accurate picture of the field covered by the rest of the book, which may be criticized by some for the amount of space it devotes to the author's own researches. But this volume is best regarded not as a comprehensive guide-book to a well-mapped territory, but as the equivalent of a travel book written by one enthusiastic explorer. What makes it worth-while is Oswald's wide range of interests, embracing neurophysiology, experimental psychology, clinical psychiatry and neurology and, particularly, electroencephalography. The E.E.G. has proved elsewhere a disappointingly blunt instrument in the investigation of electrical correlates of mental processes but comes into its own in this field in providing an objective measure of the level of cortical arousal. This has been ably exploited by Oswald himself in working on a variety of problems and by Kleitman's group in their study of dreaming. The book contains a good summary of this work.

A discussion of recent ideas on attention mechanisms in the nervous system re-emphasizes the important fact that there must be some degree of complex analysis going on of incoming signals to which one is not attending. This leads on to a consideration of dreams, hypnagogic hallucinations and imagery in general, which occupies about a third of the book. Though speculative, this is one of the most interesting sections, and contains a thoughtful and, on the whole, plausible attempt to harmonise the evidence of introspection with the neurophysiological data and with the mass of accumulated evidence on the detailed character of hallucinations occurring in association with drowsiness or in psychosis, which latter show many striking parallels.

Oswald suggests that imagery occurs particularly when the level of directed vigilance (attention) is disproportionately low compared with the level of diffuse vigilance (alertness). Such a disproportion may occur either in conditions of low general vigilance (drowsiness or light sleep) or in highly alert subjects when there is some difficult task and no relevant sensory data available and the attention is undirected. Adopting the view that all mental images are reconstructions and not reproductions of past sensory experience (these impressions being stored, after perceptual analysis, in an imageless, un verbalized form as a concept or meaning schema) Oswald holds nonetheless that perception of an image involves a similar neurophysiological response to sensation. He argues that whether such images are falsely accepted as sensory experience or not depends on whether they are accepted as compatible with what is considered reality at the time. With lowered cerebral vigilance we will accept elements which would be regarded as incompatible at higher levels of arousal. But the subject's contact with waking reality is also important. Non-veridical "sensory" images may be accepted as real either because contact with reality is attenuated (as in sensory deprivation experiments) or because lowered cerebral vigilance leads to loss of discrimination (as in the hallucinations of prolonged sleep deprivation). Hypnagogic hallucinations and dreams are related to both factors, but our degree of insight into them varies. In drowsiness, contact with reality is still intermittently maintained and we recognize the images as unreal, but at the latter end of a night's sleep, when contact with outer reality has been suspended for some time we may not do so even though the same level of cortical arousal is reached. This part of the book has been outlined here because it seems to the present reviewer a stimulating and original approach to the study of hallucinations, which merits the consideration of other psychologists. The argument is lucidly presented and the evidence well-marshalled. It remains to be shown whether it can prove a useful framework on which to design experiments.

The book includes a chapter of a more Pavlovian flavour on sleep as a provoked response, which shows that the author has been influenced by Sargant's ideas on the psychology of "brain-washing" and conversion. Oswald has interesting comments to make on many topics raised in the course of the book, such as the suggestion that Bills blocks may be graded drops in vigilance rather than all-or-none phenomena and his critique of Dement's theory of the "need to dream," but where his interest is not so obviously aroused, other topics get rather more sketchy treatment. Space should surely have been found in the last chapter on clinical disorders of sleep to mention the fascinatingly anomalous condition of akinetic mutism first described in 1941.

The book is well-printed on good paper and the illustrations are plentiful. The well-known difficulty of reproducing E.E.G. records has been tackled sensibly by printing them vertically up the page except where a short excerpt suffices. As well as ample cross-referencing in the text there is both a subject and author index and large bibliography containing many recent references which will no doubt be gratefully mined by other workers in the field. The latter includes one splendid fake reference, also referred

to in the text, which it must be supposed the author intended as an arousal stimulus, to reactivate the flagging brain-stems of his readers. Or was it, perhaps, to see if they were still awake?

A. M. HALLIDAY.

The Experimental Psychology of Beauty. By C. W. Valentine. Methuen's Manuals of Modern Psychology. Edited by C. A. Mace. London: Methuen. 1962. Pp. xi + 438. 45s.

It is a pleasure to see a book devoted to gathering together the many experimental researches on the psychology of art, music and poetry which have been published in various journals during the last half-century. That this task should fall to the lot of Professor C. W. Valentine is even more gratifying, for he has contributed no small amount to these researches himself, and published a small book on the subject over 40 years ago. He is a precise and thoughtful judge of his own and other people's work, and is well qualified to write the present book.

It starts with an introductory chapter dealing with the problems of the psychology of art and beauty in a general way. After this the author turns to colour and colour preferences, and attitudes to colours and colour combinations. A more than ordinary account is taken of variations of colour sensitivity. Forms, lines, suggested movement, balance and symmetry are important, and these and other aspects of pictorial art, which have been studied in many experiments are dealt with in several chapters. One of these chapters is concerned with reactions to modern art.

These very interesting chapters are devoted to the experimental study of musical intervals, consonance and dissonance, adaptation to discords, quarter-tone music, rhythm, melody, harmony, timbre, synaesthesia, the affective values of music, the expressiveness and meaning of music, attitudes to modern music, and so on. An unusual aspect of the book is that it deals with experiments not only on music and painting, but also on poetry.

In connection with many of these researches, it is perhaps unnecessarily modest that Professor Valentine's own name does not appear in the Index of Names, although those of all his collaborators are to be found. The concluding chapter admirably summarizes the main contents of the book. Professor Valentine, closely connected as he was with Myers, Burt, Vernon and others who have contributed much to the experimental psychology of aesthetics, and being a considerable contributor himself, has been able to write a most valuable book on this subject, for which all those who are interested in it, from whatever point of view, will be grateful.

R. W. PICKFORD.

Psychology: A Study of a Science. Vol. 4. Biologically Oriented Fields: Their Place in Psychology and in Biological Science. Edited by Sigmund Koch. 1962. New York, Toronto and London. McGraw-Hill. Pp. xxxix + 731. 97s.

In his review of the first three volumes of this mammoth compilation (this *Journal*, 1961, 13, 248-50), Sir Frederic Bartlett gave an admirable account of the background of the venture and its achievement to date. The next three volumes, we are told, are to be concerned with the empirical substructure of psychology and its relations to other sciences: this, the fourth in the series, is devoted principally to biological, and especially neurophysiological, issues. Among the topics discussed are psychology and genetics, psychophysics and neurophysiology, neuroelectric events in sensory communication, and experiment and theory in physiological psychology. There are also a number of chapters on perception, several of which transcend physiology. While some of the chapters are highly systematic, others reflect a more selective and personal view. Unlike the usual handbook, the emphasis is fairly generally on *interrelations*, not only those between psychology and other disciplines but also between different disciplines in the study of particular topics, e.g. the interrelations of physics, physiology and psychology in the study of vision. It is the Editor's evident hope that this inter-disciplinary (? supra-disciplinary) approach may help to combat the mere acquisition of knowledge in psychology and open the way to progressive discovery. Perhaps what he really means is that we need another Helmholtz.

O. L. ZANGWILL.

A Theory of Cognitive Dissonance. By Leon Festinger. London. Tavistock Publications. 1962. Pp. xix + 291. 35s.

This rather interesting study first appeared in America in 1957. The present edition is unchanged but none the less useful in making the author's distinctly bright ideas better

known in this country. Although Dr. Festinger perhaps overstates his case, his work gives good hope that what is loosely called experimental social psychology is entering a more constructive phase.
O. L. ZANGWILL.

C. A. Mace: A Symposium. Edited by Vida Carver. London. Methuen. Penguin. 1962. Pp. 134. 21s.

This well-deserved tribute to Alec Mace contains contributions by Rex Knight, E. Pratt Yule, Pearl King, Thelma Veness, R. S. Peters, S. G. Lee and John Wisdom. B. R. Singer has compiled a bibliography.
O. L. ZANGWILL.

Recent Soviet Psychology. Edited by N. O'Connor. Oxford: Pergamon. 1961. Pp. 334. 70s.

In his preface to this collection of articles, Thouless points out that experimental enquiry in the U.S.S.R. has been directed towards some problems that have been relatively neglected in our own laboratories. The selection of 16 articles, seven under the heading of Physiology, four under Speech, and five under Education, was made from a list of recommendations from the Psychology Section of the Moscow Academy of Science, and the final selection then made by the editor gives a very valuable coverage of recent contributions, mainly in *Voprosi Psichologii*, and representing the major divisions of Soviet Psychology, such as educational psychology, Pavlovian typology, thinking, speech, sensation and perception, and attitudes or sets. Some work on the verbal control of behaviour in subnormal patients is also included, but experiments on animal conditioning from the Pavlovian laboratory are excluded, these appearing mainly in the *Journal of Higher Nervous Activity* (see Razran's recent reviews in *J. comp. physiol. Psychol.*).

Preceding the selection of articles, the editor has contributed a useful introductory review of contemporary trends in Soviet Psychology, in which he points out that the tendency to capitalize on the Pavlovian approach to the exclusion of others is no longer prevalent. The broader approach here represented is also indicated by the amount of reference to non-Soviet psychological and physiological work.

The contents of several of the articles are likely to be of special interest to psychophysiological experimenters, for example, those dealing with electrophysiological and myographic studies of speech and voluntary movements, and the contributions on conditioning in which the rôle of the afferent system is particularly emphasized.

K. R. L. HALL.

Creativity and Intelligence: Explorations with Gifted Students. By Jacob W. Getzels and Philip W. Jackson. London and New York: Wiley. 1962. Pp. xvii + 293. 49s.

Professors Getzels and Jackson set out to explore the nature of intellectual ability. They use the simplest of means, yet their results are startling. They contrast two groups of clever American schoolchildren: the "High IQ's" and the "High Creatives." The High IQ's are children who score very well on conventional IQ tests, but relatively poorly on tests which are open-ended (e.g., "How many uses can you think of for each of the following everyday objects?"). The High Creatives are the reverse; they are very good at open-ended tests, but relatively low in IQ. There was a mean difference of as much as 23 IQ points between the two groups. Both their similarities and their differences are intriguing. Despite the difference in IQ, the two groups were equally successful academically. They seem to be equally hard working; but there are, nonetheless, marked differences in attitude and personality between them. Relatively, the High Creatives are more given to humour, violence, incongruity and seeming irrelevance in written work and drawing; less conforming to school values; less liked by teachers; less likely to come from critical homes; and so on. The case material makes it clear that these differences are not superficial, but reflect two or more different varieties of personality.

These results have important practical implications. First: that faith in the IQ as a measure of "basic" or "essential" intelligence is misplaced. Second: that—either through misplaced faith in the IQ, or through too rigid a method of teaching—schools may be discouraging precisely those children most likely to be original as adults. There is also an uncomfortable implication for mental testers themselves: that their profession is in poor epistemological health. For 30 years, the testing empire has grown steadily larger and more sophisticated, yet the present discovery—simple and accessible though

it is—has been missed. There are signs, in other words, that scholasticism has set in, that mental testers are using their paraphernalia to keep awkward facts, and the individuals that embody them, at a safe distance.

In pleasant contrast, this book is *not* boring; it uses statistics intelligibly; and it shows sensitivity to qualitative differences—all of them virtues we should be able to take for granted but cannot. Above all, its authors realize that the psychologist who tries to understand human behaviour is well advised to look closely at human beings. Even so, the book does have its weaknesses. It seems a mistake at this stage, for instance, to define "creativity" in terms of test scores. This begs the question of creativity as badly as the Intelligence Quotient begs that of intelligence. There is also a chapter about moral and emotional qualities. Little is made of them, and they perhaps deserve a book to themselves. Lastly, the book seems too short. The authors use individual cases plentifully; but only as illustrations. One is not shown the exceptions to the rule. A systematic treatment of case material would take a lot of space; but for this reason as much as for its intrinsic interest, one would like the book to be three or four times longer than it is. In general, though, this a splendid piece of research; and a good omen perhaps—that mental testers are drifting back into contact with humanity.

LIAM HUDSON.

Programmed Learning: Theory and Research. Selected Readings edited by W. I. Smith and J. W. Moore. London: Van Nostrand Co. 1962. Pp. 240. 15s.

For some time the only two books on programmed teaching which have been generally available have been the all too hasty collection of articles edited by Galanter, and the most useful volume of Lumsdaine and Glaser. There are now several journals which cater specifically for articles on teaching machines and a number of books are beginning to appear. This edition represents one of the number being a compilation of 16 papers, most making their second if not their third and fourth appearance.

The editors attempt to unify their volume by grouping their selections into three sections. Part I presents the development of ideas and some of the relevant psychological theory, Part II discusses the method of programming, and Part III samples current research on automated procedures. Inevitably an editor's selection never agrees with a reviewer's and this volume firmly maintains the tradition. There seems little reason for including some of the older and longish papers, some of the so-called research papers are distinctly lightweight, and there is no contribution from communication engineering. Much of the writing on theory shows little subtlety and less learning and the rhetorical style of some articles is ill-suited for accurate exposition. The appendix has some useful information but there is no point in a glossary that defines reinforcement as "the process of providing the subject with immediate feedback or information regarding the *success or failure* of his performance" (reviewer's italics). One major theoretical issue is precisely about this point.

This is a handy size and cheap introduction for any reader who wants to know something about programmed learning, but it is to be hoped that papers in this field which are reprinted will reach a higher level than do the majority of this selection. H. KAY.

Cognition. By F. H. George. London: Methuen's Manuals of Modern Psychology. 1962. Pp. 309. 32s. 6d.

Dr. George's main concern in this book is with theory-building. "I have attempted," he says, "to lay out clearly some of the evidence relevant to the finding of appropriate models and theories for predicting human behaviour" (p. 9). To talk most helpfully about cognitive processes, so he claims, we need a modified version of behaviourism; its main characteristic, if I have understood him correctly, is the stipulation that sentences containing words such as, e.g., "believe" or "think" require to be interpreted (or re-interpreted?) in terms of behaviour and not as though they referred to something of which we are aware by "introspection." The concepts which emerge as being of special interest include "feedback," "selective reinforcement," "storage," "scanning," and "categorization." "Learning . . . is a system of association and storage of a stochastic type. The principles of input depend both on specialized analysing mechanisms and classification, and all the terminology of perception and learning can be reinterpreted on such a simple base" (p. 294). The book is in effect a plea that we should theorize about existing data on perception and learning in these terms; and in the early chapters the researches of Pavlov, Köhler, Hull, Tolman, Ames, Lorenz and others are presented with this objective in view.

My first reaction on reading this book was one of extreme exasperation. Dr. George's basic programme seems to me both correct and important, and his versatility (he ranges effortlessly from Berkeley's philosophy, via ethology, to the programming of computers) is highly commendable. But the book is studded with "howlers" and inaccuracies of every conceivable kind. Thus the distinction between "primary" and "secondary" qualities cannot be adequately explained, as Dr. George supposes (pp. 130-1), by saying that "primary" means "objective" and that "secondary" means "subjective." Nor did Berkeley believe that a substance was a set of qualities (p. 131); *we ourselves* are substances, on Berkeley's view, since we can cause things to happen. In general, Dr. George's philosophizing comprises a series of *obiter dicta*, some of which are very illuminating, but many of which are too cryptic to be intelligible. Nor is his psychology any more disciplined. Thus the summary of Michotte's work, which occupies less than seven lines, is utterly inadequate (p. 157), and even Weber's law is formulated in a curiously ambiguous way (p. 14). On matters of style and layout the book is a sheer disaster. Thus the bibliographical references contain errors and inconsistencies of dating as well as downright mistakes (e.g. Professor M. D. Vernon's *A Further Study of Visual Perception* is cited as *A further Analysis of Perception*), and on at least three occasions (on pp. 97, 149, and 243) the references promised in the main text do not appear at all in the bibliography. There are errors of grammar, e.g. unattached participles on pp. 70, 72 and 197, and the use of "data" as a singular (pp. 142, 163 and 294), and there are even some misspellings, e.g. "a consensus of opinion" (p. 260), "transmissibility" (p. 178), "transcience" (p. 175), "leuchotomy" (p. 29), and "Ebbinghouse" (p. 20).

My general conclusion is that as a textbook for beginners or laymen the book is too misleading to be recommended. Those that can take care of themselves, however, will find it both interesting and exciting.

T. R. MILES.

Brain and Behaviour in Cephalopods. By M. J. Wells. London: Heinemann. 1962. Pp. 171. 16s.

Over the past eight years or so a detailed study of the behaviour of the octopus has been undertaken by Professor J. Z. Young and various collaborators, the work being done at the Stazione Zoologica at Naples. In this book M. J. Wells summarizes the main findings which have been obtained to date. The book starts with a very brief description of cephalopods in general, followed by a short discussion of some of the problems of keeping different species in the aquarium. There follow chapters dealing with visual and tactile discrimination in the octopus, and then two sections on the effect of lesions and stimulation of the C.N.S. on learnt and innate behaviour. Finally there is a brief description of *Nautilus* (a "living fossil"), and an appendix discussing possible reasons for the relative lack of success of the cephalopods as a group.

The author in his preface states that his purpose is to describe the observations made, and not to discuss any of the theoretical considerations which have arisen from the work. The book thus consists essentially of a straightforward description of the different experimental findings. Within this limitation the book fulfils its function, though there are some inaccuracies (for example, on page 120 it is stated that the emptying of the crop is unusually rapid in animals having vertical lobe lesions: Young's original paper, however, states that this is not the case). Some sections are also oversimplified, particularly that on visual learning and the effects of vertical lobe lesions, but the main experimental findings are clearly set out and easy to grasp. The Heinemann studies in Biology are apparently designed for sixth forms and undergraduates, and Wells succeeds in summarizing the work in a convenient form for such people. However, the approach also reduces the interest of the work very considerably, for it gives no indication of the purpose or the general approach underlying most of the experiments. The book thus gives an erroneous impression of a series of disconnected experiments. This is particularly unfortunate as another review of the work on the octopus has just been published and a very detailed book is scheduled for the near future, so that a factual account at this point serves little purpose. Under the circumstances it is a pity that the author, by deciding to restrict himself to a simple redescription of published work, has given a poor idea of the great interest and potential which exists in this field.

W. R. A. MUNTZ.

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